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THE QUARTERLY JOURNAL OF
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ACCURACY OF AIMING IN LINEAR HAND-MOVEMENTS

BY

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Six subjects took part in an experiment which consisted of drawing, in a horizontal plane, a number of lines of different lengths and in different directions. The lines were drawn from a starting-point towards a target-point, and the instructions and previous practice were designed to ensure that the lines were drawn in one movement without appreciable amendment during the drawing. The direction and length of the lines were both found to affect the accuracy of aiming, and certain directions showed a persistent bias in the distribution of their errors. Closing the eyes just prior to and during the drawing of the lines had the result of diminishing the effect of changes in direction and length, but it increased the bias. In attempting a theoretical analysis of the results, some of the limitations imposed by human anatomy have been considered in an elementary way. The general conclusion is that, in this task, the effects of altering length, of altering direction and of opening or closing the eyes can, to a great extent, be plausibly ascribed to the particular anatomical nature of the human operator, provided one makes certain simple assumptions about the theory of movement control.

INTRODUCTION

Linear hand-movements have been studied in a number of different experimental contexts. In most of these the task involved the accuracy of the final position, e.g. Thorndike (1931), Peters and Wenborne (1936), Macpherson, Dees and Grindley (1948), Brown, Knauft and Rosenbaum (1948), Brown and Slater-Hammel (1949).

On the other hand, there is not so much information available about simple aiming at a target, where the criterion of success is the closeness of the track to the target, and where the hand-movement may be allowed to proceed beyond the target-distance, if necessary. In experiments by Fitts (1947), results were obtained on the accuracy of reaching with a stylus to an array of targets arranged round the subject. The movements were probably not linear and their nature was quite free in three dimensions. In a series of experiments by Corrigan and Brogden (1948, 1949) accuracy of aiming was assessed during the movement, as it were, rather than as an end-result, by counting the number of contacts with the sides of a linear track. The task was pursuit in nature and the speed was comparatively slow. In both these sets of investigations, the direction of movement was found to play an important part in the resultant accuracy. Siddall and Holding (1956) studied a task in which errors of both positioning and aiming could be made. The movements were fast, but the four directions which they chose did not show significant differences in accuracy.

The present experiment was designed to study the accuracy of aiming in a horizontal plane, when the direction and length of the path could be altered. To obtain further information on the way in which the movement was being controlled, it was arranged to carry out the experiment both with and without visual cues during the time of drawing the lines. Since only accuracy of aiming was in question, the line was defined by a starting-point and a target-point (not an end-point), and no attempt was made to measure errors of positioning in the direction of the path. The accuracy was recorded by examining whether the line went to the right or left of the target-point and by how much. Since internal details of the movement were not being investigated, it was found convenient to have the lines drawn at a fairly fast rate, so that they might be as straight and as free from secondary corrections as possible.

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* Now at the Physiology Department, Edinburgh University.

Method

Subjects

The six subjects who took part were naval ratings of between 20 and 23 years of age, with at least 3 years of service. Only one had done any manual precision work (engineering tool designing). All were right-handed and had normal eyesight.

Task

At each session the subjects were required to draw with pencil on paper, 128 lines of four different lengths and in eight different directions, i.e. four lines for each direction and each length. They had five daily practice sessions in one week, one practice session at the beginning of the next week, followed by the two experimental sessions on consecutive days. In the first of the experimental sessions the eyes were open all the time; in the second they were closed just prior to and during the actual drawing of the lines.

The eight directions in which the lines were drawn were at angles of 0°, 35°, 90°, 145°, 180°, 215°, 270°, 325°, where 0° was directly away from the subject and the angles were measured in an anti-clockwise direction. The diagonal directions were relatively close to the directions from and towards the body, rather than symmetrically placed, because preliminary experiments had shown that there was a critical direction in this vicinity and it was desired to close in on it. The four lengths were 16, 20, 24 and 32 cm. Again the choice was influenced by trial experiments.

For each direction and each length a sheet of paper was prepared with pinpricks, so that four lines of the same direction and length could be drawn by joining up diametrically opposite points of two rows of pinpricks. The total of 32 sheets (one for each of the eight directions and four lengths) was randomized for direction and length and presented to the subject one at a time. The randomization was the same for each subject on any one day, but differed from day to day. The angle associated with each direction was called out to act as a cue for starting to draw the four lines on any sheet. The subject had a plan of the directions with their angles on a separate sheet of paper beside him.

The instructions which were given at the beginning of each practice session and the first experimental session were: "When you hear the angle called out, decide which is the correct direction and where you will need to start. Put your pencil on whichever of the four corresponding dots is highest or most to the left. Plot out mentally where you want to go in order to draw a straight line to the diametrically opposite dot. Then, keeping looking at this opposite dot, draw the line with as straight a sweep as possible. Don't worry if your pencil goes off the edge of the paper. You may not hit the dot but aim for it and keep the line as straight as possible. Repeat this for the other three pairs of dots on the sheet. The order in which you draw the four lines on a sheet should be from left to right and from above downwards. When you have finished with one sheet it will be removed and you will be given a new one, and a new angle will be called out."

The instructions for the second experimental session (in which the eyes were closed just prior to and during the drawing) were the same except that, instead of the fourth sentence there was substituted: "Look upwards; close your eyes; count—one, two, three; then draw the line with as straight a sweep as possible."

On both sessions, the subject could view the result of his drawing immediately afterwards, before proceeding to the next sheet. Although no instructions were given to increase the speed, it was found that by the end of the practice sessions all the subjects were taking less than half a second to draw any line.

Environment

Each sheet of paper was presented to the subject by laying it on a rectangular pad. The sheets of paper and the pad measured 32½ cm. by 20½ cm. The centre of the pad was in all cases, at a distance of 30 cm. from the centre of the front of the subject's body, but the narrow edge was nearer the subject for directions 0°, 35°, 145°, 180°, 215° and 325°, while the wide edge was nearer for directions 90° and 270°. This arrangement and the shape of the pad were adopted because of the angles used in the experiment. The perceptual framework, within which the lines were drawn, was therefore slightly different for the 90° and 270° lines. However, some preliminary experiments with different sizes of paper indicated that, provided sufficient practice had been given, such an alteration in the relative dimensions of the framework appeared to have no significant effect on the accuracy of the drawing.

The subject was seated on a backless stool which was at a height of 51 cm. above the floor. He could rest his arms, if desired, on the table, which was 77 cm. high and situated at the approximate centre of the room. The surface of the table was draped with a dull black cloth. The room was blacked out, and the only source of illumination was a 40-cm. high table lamp with a 60-watt bulb. A cylindrical shade enabled a circular pool of light to be cast on the working surface, and this circle had a diameter of approximately 45 cm. in the area of full brightness.

The subjects were instructed to keep their shoulders and trunk squarely to the table during the task, and they were observed closely to see that this was done. But no form of harness was used, as it was thought that its presence might introduce further complications.

Scoring

The accuracy of aiming was assessed in two ways in order to emphasize two different aspects of the task. Firstly, the perpendicular distance of each line from the target point was measured to the nearest millimetre, and this quantity was summed over the four lines on the sheet, irrespective of which side of the target-point was involved. The result was taken as the "error" for a particular direction, a particular length and a particular subject. Analysis of variance procedures were carried out on these error scores.

Secondly, each line was also assessed on the basis of whether it passed clockwise or anti-clockwise of the target-point. For each direction, length and subject, it is possible to count the number of attempts which have a clockwise error and the number which have an anti-clockwise error. The ratio of these two numbers gives an indication of any "bias" that may be present in the distribution of the attempts around the required line.

In the subsequent statistical work, results whose probability of occurrence by chance alone is over 5 per cent. are reckoned as not significant. The phrase—"significant at the x per cent. level of probability"—is taken to mean that the probability of achieving the particular result by chance alone is less than or equal to x per cent.

RESULTS

Vision

A preliminary analysis of variance applied to the complete data showed that the main effects due to differences between subjects, between lengths, between directions and between the two visual conditions were all significant (at the 5, 1, 1 and 1 per cent. levels of probability respectively). None of the interactions was significant. The effect of closing the eyes was to multiply the errors four to ten times, and the corresponding F-ratio for the variance due to the difference between the two visual conditions had the very large value of 209·32.,Because of this, and because there was no interaction between visibility and any of the other variables, it was thought appropriate in what follows to analyse separately the figures for eyes open and eyes closed (Table I) .

Length

It is clear from Figure 1 that increasing the length increases the error, and Table I shows that the difference in errors between lengths is significant for both eyes open and eyes closed.

It also appears that there is a sudden large increase in error in passing from length 24 cm. to length 32 cm. It is possible to test this by dividing the lines into a "short" group—16, 20 24 cm. in length, and a "long" group of 32 cm. (The terms "short" and "long" will subsequently be used in this particular sense.) This subdivision can be introduced into the analysis of variance and it is then found that the overall significant difference due to length resides principally in the difference between the short and long groups in the case of eyes open, but that it is not possible to make this statement statistically significant for eyes closed.

TABLE I

ANALYSIS OF VARIANCE

	Variable	*S.S.*	*D.F.*	*M.S.*	*F-ratio*	*Significance*
EYES OPEN	Subjects (S)	1412	5	282	10·85	5%
	Directions (D)	779	7	111	4·27	1%
	Lengths (L)	286	3	95	3·65	5%
	Between groups	284	1	284		
	Within groups	2	2	1	284·00	1%
	S × D	1178	35	34	1·31	. . .
	S × L	677	15	45	1·73	. . .
	L × D	724	21	34	1·31	. . .
	Between groups	362	7	52		
	Within groups	362	14	26	2·00	. . .
	Residue	2708	105	26		
	Total	7764	191			
EYES CLOSED	Subjects (S)	2071	5	414	1·28	. . .
	Directions (D)	5101	7	729	2·26	5%
	Lengths (L)	3228	3	1076	3·33	5%
	Between groups	2365	1	2365		
	Within groups	863	2	432	5·47	. . .
	S × D	16816	35	480	1·49	. . .
	S × L	2824	15	188	0·58	. . .
	L × D	8614	21	410	1·27	. . .
	Between groups	6858	7	980		
	Within groups	1756	14	125	7·84	1%
	Residue	33917	105	323		
	Total	72571	191			

The "groups" refer to the long-short classification.

Direction

Table I indicates that the difference in errors between directions is significant for both eyes open and eyes closed. Error has been plotted against direction for each of the four lengths in Figure 2, and an extra graph has been added showing the total error over the three short lengths.

(*a*) Although, there is no overall interaction between length and direction, the subdivision into short and long groups gives the possibility of comparing the relative significance of the different sub-interactions. It is found that the interaction between length and direction is significant for the between-group variance with respect to the within-group variance for eyes closed but not for eyes open. One might say that the error-direction pattern for the long lines is significantly different from that of the short lines for eyes closed (see Fig. 2). It may be noted that the same general trend of patterns is apparent for eyes open, although the results do not reach statistical significance.

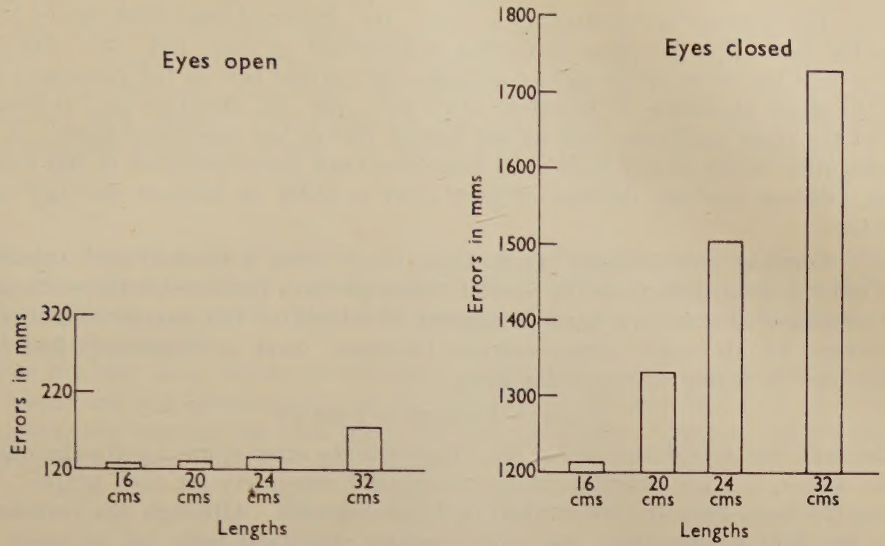


FIGURE 1

The values of the total errors at each of the four lengths.

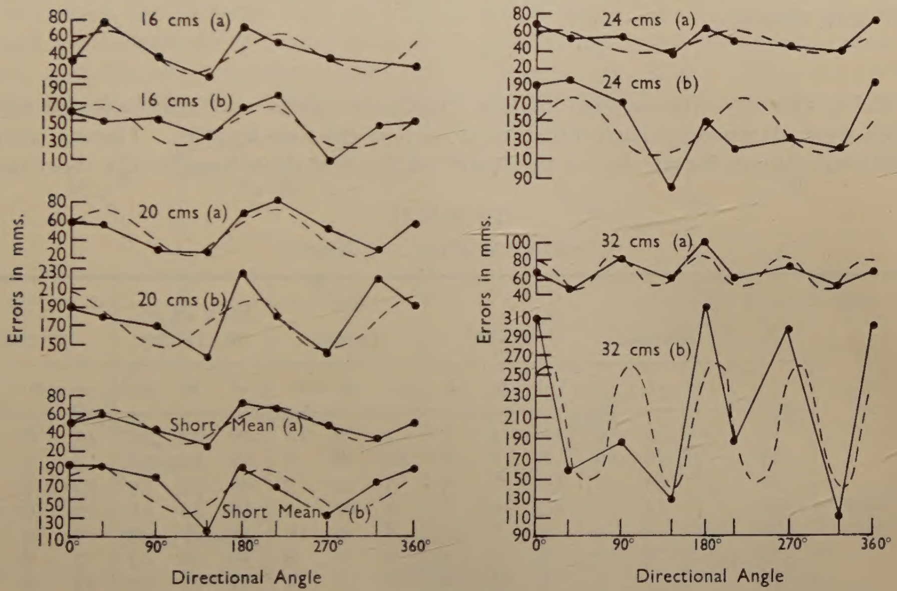


FIGURE 2

Error as a function of direction for: (*a*) eyes open, (*b*) eyes closed. The "short mean graph" gives the values for the means of the 16, 20 and 24 cm. graphs.

(*b*) Although the overall difference between directions is significant, it is found that thirty of the thirty-two pairs of opposite directions show no significant difference between members of a pair, as reckoned by a standard error and *t*-test. This biphasic character might, of course, have been expected on *a priori* grounds. In addition, there appears to be a quadriphasic character to the long line graphs, for in twenty-one of twenty-four possible cases, no two of directions 35°, 145°, 215°, 325°, nor of 0°, 90°, 180°, 270°, differ significantly from each other.

(*c*) The approximate position of a "best" line, or line of least error can be found from the data. For the phase consisting of directions 35°, 90°, 145°, 180°, direction 145° is best for seven of the eight conditions and second best in the remaining one. For the phase consisting of directions 215°, 270°, 325°, 0°, direction 325° is best for five of the eight conditions and second best in two of the remaining three. A full investigation would clearly need more directions than have been used in this experiment, but one can say the line of least error is likely to be near the 145°–325° direction.

(*d*) Corrigan and Brogden (1948, 1949) while using a method and apparatus that differ substantially from the present technique have produced evidence to show that precision of arm and hand movement is related to the angular direction of movement by an exact trigonometrical function. Such a hypothesis has been tested here by fitting curves of the form

$$y = a + b \cos 2x + c \sin 2x$$

to the data (the dotted lines in Fig. 2). Here y is the error score, x is the directional angle, and a, b, c are constants to be determined separately for each graph. The fitting has been done by the method of Least Squares. Although the correlation with the data is suggestive, the more rigorous statistical tests fail to reach the required level of significance. It has therefore not been possible to prove that the observed cyclical relationship between errors and direction is necessarily of a strict sinusoidal nature.

Bias

Table II shows the number of hits, clockwise misses and anticlockwise misses which were obtained for each direction at each of the four lengths. There is also an additional column for the sum of the scores for the first three lengths (the short lines).

TABLE II

NUMBERS OF HITS AND MISSES

	A	16 cm.			20 cm.			24 cm.			Sum of 16, 20, 24 cm.			32 cm.		
		—	0	+	—	0	+	—	0	+	—	0	+	—	0	+
EYES OPEN	0°	8	9	7	11	5	8	7	6	11	26	20	26	10	6	8
	35°	2	6	16	8	8	8	10	8	6	20	22	30	9	7	8
	90°	8	13	3	12	8	4	5	12	7	25	33	14	13	7	4
	145°	7	13	4	12	10	2	9	10	5	28	33	11	6	6	12
	180°	11	4	9	6	8	10	8	5	11	25	17	30	8	3	13
	215°	12	6	6	9	6	9	9	7	8	30	19	23	7	8	9
	270°	11	8	5	11	11	2	12	8	4	34	27	11	12	2	10
	325°	10	9	5	6	10	8	6	12	6	22	31	19	11	7	6
EYES CLOSED	0°	4	3	17	9	0	15	6	0	18	19	3	50	6	2	16
	35°	3	5	16	5	5	16	8	1	15	16	11	47	10	6	8
	90°	10	3	11	11	3	10	10	3	11	31	9	32	14	2	8
	145°	13	5	6	15	7	2	13	4	7	41	16	15	10	3	11
	180°	7	1	16	9	1	14	8	0	16	24	2	46	9	3	12
	215°	11	4	9	10	0	14	17	1	6	38	5	29	14	0	10
	270°	15	5	4	17	1	6	17	5	2	49	11	12	17	2	5
	325°	17	0	7	14	5	5	18	2	4	49	7	16	7	7	10

A = angle. 0 = hit. — = anti-clockwise miss. + = clockwise miss.

In analysing first the results for the short lines, it is apparent from the table that the largest biases occur when the eyes are closed. In the latter condition, the pattern of biases is remarkably constant over the three lengths. Apart from directions 90° and 215°, each of the remaining directions has precisely the same sense of bias for each length. The biases for the combination of the first three lengths show the same constant pattern. It may therefore be said that the pattern:—

0°	35°	90°	145°	180°	215°	270°	325°
+	+	...	—	+	...	—	—

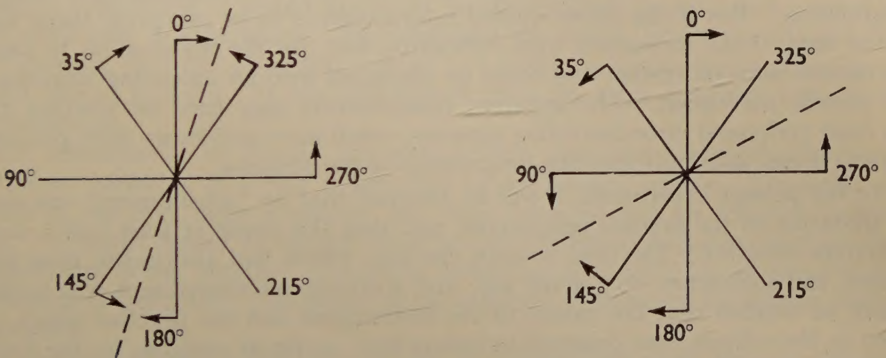
(where + denotes a clockwise bias and — an anti-clockwise one) fits all the results in the eyes closed condition. In the eyes open state, the same pattern fits thirteen of the eighteen results for the three separate lengths, and all the results for the combination of lengths.

For the long lines, the establishment of a pattern must be more tentative, since the biases are less pronounced. It will first be noted that there are only three disagreements between the eyes open and the eyes closed condition. Hence, apart from directions 215° and 325°, the pattern:—

0°	35°	90°	145°	180°	215°	270°	325°
+	—	—	+	+	...	—	...

fits all the results for the eyes closed condition and all but one of the results for eyes open.

Therefore, for both the short and the long lines there are at least six directions in each case which show a uniformly predominant bias in the distribution of their errors, and this effect is most marked when the eyes are closed. These results are summarised in Figure 3. The short line figure shows a high degree of symmetry,



Short lines

Long lines

FIGURE 3

Diagrammatic representation of the bias for each direction. The arrows indicate the direction of the errors at the target-points.

the effect of which is to suggest that the various directions are "pulled in" towards a line situated slightly anti-clockwise of 145°–325°. The long lines show almost as high a degree of symmetry, with a pulling-in towards a line probably slightly clockwise of 145°–325°. The special line towards which others are pulled is certainly in the neighbourhood of the line of least error (that one common to both the short and long lines). Reasons are advanced in the Discussion for believing that the two lines are identical.

Discussion

As an initial simplification one may assume that the previous practice has eliminated most of the more obvious sources of error due to misinterpretation of the visual field or to faulty assessment of the physical demands of drawing with pencil on paper. There will still, however, be possible sources of error in the interpretation of information fed back from the hand and arm.

In particular, one must ask whether the subject can correct mistakes as soon as they are made, given the necessary motivation. Woodworth (1899), in studying the relationship of speed and accuracy, concluded that movements made at a rate faster than two a second could not be subject to amendments based on visual cues. They were governed, he said, by the "initial impulse" rather than by "current control." Peters and Wenborne (1936) made the same distinction in a slightly different terminology when considering the way in which velocity changes along the path of a movement. The work of Vince (1948), Taylor and Birmingham (1949) and Chernikoff and Taylor (1952) on responses to abrupt changes of visual and kinaesthetic stimuli indicates that the time limit of approximately half a second also applies to amendments based on kinaesthetic cues. The theoretical basis of the time limit has been discussed extensively by Welford (1952). However, Gibbs (1954), in querying a pure "impulse discharge" theory of movement control, has put forward the view that kinaesthetic feedback of rate of movement is proceeding continuously in any task such as the present one. He considers that this feedback can be integrated from time to time to give essentially intermittent information on position.

It can, of course, hardly be denied that there is some current feedback of kinaesthetic data, and doubtless this forms the basis for organizing subsequent programmes. But if the above quoted information is to be accepted, there would not be time, if the movement were sufficiently fast, for the information to pass to the central nervous system and there be organized into an amending response for the current movement. The apparent disagreement may turn on whether there can exist peripheral error-correcting systems, which have previously been pre-set by higher centres and built into the programme of the response.

In the present experiment, it will be assumed that by "amendment" one means an alteration to the original programme, and that this needs at least half a second to become effective. The time to draw the lines was in fact always less than half a second, and sometimes very much less, and therefore no amendment was possible. It will be recalled that the nature of the instructions and the practice which were given to the subject were designed to ensure that, as far as possible, by the time of the experiment itself the subject was making movements of precisely this fast, straight nature.

Such movements have been called by Craik (1947) and others—"ballistic." But this term, as used originally, e.g. by Stetson (1905) referred to cases where the moving member was uncontrolled by muscular impulses in the direction of its course, for the greater part of that course, i.e. it is flung, as an almost inert object, by one set of muscles and is "caught" by the antagonists. To call the present type of movement "ballistic" may suggest that the limb is not subject to any control in its path, whereas there may be plenty of control, but of a pre-set nature, which cannot be changed during the movement.

However, it cannot be assumed that the resultant accuracy is necessarily entirely dependent on the accuracy of the original programme, or "initial impulse." The programme itself may not remain intact; it may be subject to some kind of drift between its planning and its execution. It is well-known, e.g. by golfers that once a movement has been planned, the absence of continuous checking before it is carried out has a deleterious effect. One method of providing the opportunity for this is to introduce a time-lag into the system, and this was noted by Woodworth (1899) in a study of repetitive movements. He found that when the velocity of the movements was kept constant, accuracy diminished if the interval between movements was increased. He attributed this to an increase in variability of the primary response due to the delay. Macpherson, Dees and Grindley (1949) reported that, in a variety of tasks, a shorter interval between performances (down to about 1 or 2 sec.) led to greater accuracy than a longer interval, when knowledge of results was given.

Now Vince (1948) and Taylor and Birmingham (1949) have shown that the absence of visual check from the moment of initiating a movement has no effect on its accuracy, provided the movement is of the fast nature described above. Hence the vital time is between the completion of the planning and the beginning of the movement. In the eyes open condition, there is the opportunity for visual checking, whether all the subjects take advantage of it or not. But in the eyes closed condition, no checking can take place in the crucial time, for the subject is engaged in shutting his eyes, raising his head and counting up to three. In other words the large difference between the errors for eyes open and eyes closed is not because one is constrasting a situation with both visual and kinaesthetic control with one that has only kinaesthetic control. The significance of the eyes closed experiment was not so much the simple lack of vision as the two facts: (*a*) there was a delay between the completion of planning what to do and the execution of the plan, (*b*) during this delay there was no opportunity for visual reinforcement of the plan. In a more extensive experiment it would no doubt be illuminating to vary and control the delay time over a wide range. The object of the simple procedure adopted here was to ensure that the delay was related in a fairly constant way to the subject's general rate of activity. It was sufficient to show the large difference between the two conditions.

The consequence of closing the eyes is then that the programme begins to deteriorate, probably in a way determined by anatomical factors. The latter still operate in the eyes open condition, but their consequences are allowed for more easily. It might indeed be thought that in both conditions sufficient practice could eliminate any of the ordinary anatomical difficulties that could be encountered in a simple task of this kind. The results of the experiment, however, are contrary to this expectation.

It has already been noted that opposite directions have approximately the same error, as would be expected from an anatomical standpoint. One might further expect that those directions which involved movements of the least mechanical complexity would have the least error. The commonest type of complexity is the involvement of a large number of joints. One cannot reduce the number too much or one would be unable to follow straight line paths at all. But, in order to minimize error, a start might be made by restricting movement at the more proximal joints in order to have the simultaneous benefit of a reduction in the mass of the actual moving part of the limb. It may also be the case that the more proximal joints are less efficient, and therefore more suitable for immobilization, although the evidence of Goldscheider (1899) on the thresholds for appreciation of movement at shoulder, elbow and wrist might seem to point in the opposite direction.

One might therefore expect that improved accuracy could be achieved in the present experiment by partially immobilizing the shoulder-joint. The most efficient way to do this would probably be to press the upper arm against the body, for then the proprioceptive impulses from the skin of the arm and body would be assisting those from the joint receptors to give the information necessary for retaining the arm in position. In addition, gravity would be having an effect in maintaining the posture. If these suggestions are adopted and the elbow is flexed, allowing the lower arm to swing freely about the elbow joint in a horizontal plane (with the humerus only moving about its long axis), then a few corrective movements at finger and wrist will enable the hand to draw a straight line, and the latter will be in approximately the position 145°–325°. This is, in fact, the position of the line of least error, as determined by the experiment.

In the experiments of Corrigan and Brogden (1948, 1949) the subjects are asked to track a moving target in different directions, as it describes a linear course of 40 cm. at a rate of 3 cm. per sec. Error was measured by the number of contacts with the sides of the track which was 0·4 cm. wide. Thus the nature, the length, the speed and the freedom of the movement, as well as the type of error scored, all differed from those of the present experiment. In fact the only similarity appears to be the anatomical nature of the movement. Nevertheless their easiest line at 135°–315° is very close to that of the present experiment. Although an anatomical explanation can give some account of the position of the line of least error, it does not suggest that the relationship between error and direction will be anything more than roughly cyclic. The exact sinusodial curves of Corrigan and Brogden are still difficult to explain.

The experimental results on the existence of bias raise an additional point. The presence of a line of least error at once suggests that if the programme for any other line should deteriorate it will tend to drift towards the position of the easiest line. This may take place in the formation of the programme or at a later stage, especially if there is a lack of visual checking. In either case, one can think of the change as a discarding of complication, if one accepts the view that the line of least error is the one that is least complex anatomically. The lines nearest to the line of least error will have a very similar programme to it and might therefore be thought to be most subject to the effect, while a line which is roughly equidistant—in an anatomical if not a geographical sense—from the two opposite directions of the line of least error will be "pulled" equally by both, and should show very little resultant effect. One may put this point in another way by saying that the subject, where possible, keeps his elbow to his side and pivots his lower arm at the elbow joint, and that the inappropriateness of this tendency for certain directions gives rise to the typical bias.

These theoretical predictions accord fairly closely with the experimental results. For the group of short lines, the directions are biassed towards a line in very much the position of the line of least error. On the whole the bias is large for directions close to this line and is small for directions at approximately right angles. For the long lines, the effect is not so great but shows the same general trend.

Brogden (1953), using the same experimental set-up as Corrigan and Brogden (1948, 1949) found no evidence for a bias in any of the eight equally-spaced directions which he studied. But it must be remembered that the greatest biases in the present experiment were found when the eyes were closed, although those of the eyes open condition followed the same pattern. In Brogden's experiments the subjects had their eyes open. In addition there was immediate knowledge, kinaesthetic if not visual, of when an error was being made and also enough time to correct it. It is probable therefore that these characteristics, while still allowing error to take place, might have tended to damp out any tendency to bias.

The simple anatomical approach which has been outlined above can also be invoked to give some explanation of the error-length relationship. It seems likely, for example, that the 32-cm. line cannot be drawn easily with the upper arm close to the side, and the necessary change of posture may account for the discontinuity in the error pattern at or just short of this length.

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TACTUAL CHOICE REACTIONS: I

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The purpose of the experiment was to observe the effect of varying the number of alternatives on choice reaction time in a task having an initial high degree of compatibility. The stimuli were presented as vibrations of relay armatures to 1, 2, 4, or 8 fingers separately, and the response was to depress the armature by the finger so stimulated. The results showed a difference between simple reaction time and two-choice times, but no systematic differences among 2, 4, or 8 choice times. The implications for further research are discussed.

Introduction

Although the role of "naturalness" in relation to reaction time work was discussed as early as 1877 by v. Kries and Auerbach, it is only during the last 15 years or so that the subject of optimum display-control relationships or compatibility has been studied in any detail. In general, it had been found that the better the relationship, the more efficient the response. The whole topic received a new impetus when the application of information theory concepts enabled one for the first time to say rather more than that reaction time increases when the number of alternatives are increased. Hick (1952) had shown that both the data obtained by Merkel in 1885 as well as his own showed a lawful relationship: in effect, the increase in reaction times was shown to be proportional to the increase in information transmitted. These findings have been confirmed in a variety of settings since. It should be noted that the lawful relationship had been demonstrated by applying measures derived from Information Theory. On the one hand this naturally gave rise to the hope that one might be able to build quantitative models of choice behaviour. On the other hand, one now had a common criterion by which to compare the results of different choice-reaction experiments: the amount of information transmitted per second. When this was done (cf. Bricker, 1955; Crossman, 1956) it was found to differ among different experiments. Both these authors noted that one of the factors which could account for these differences was the particular display control-relationship used in different experiments. On the whole, the better this relationship—or the greater the "compatibility-effects," the lower the reaction times and the smaller the differences between reaction times with increases in amount of information transmitted. In his paper Crossman (1956) confirmed this conclusion experimentally by using a simple and a more complex display arrangement to present his subjects with varying numbers of alternatives. Since then Alluisi, *et al.* (1957) have shown there to be only small increases in reaction times when the stimuli were arabic numerals and had to be responded to verbally, and Pierce and Karlin (1957) found no effects on reading rates when vocabulary size was varied over a wide range.

The conclusions of Bricker and Crossman taken together with more recent experimental findings suggested that it should be possible to take the process one step further for manual responding. If the stimuli could be presented in a suitably "encoded" manner it should be possible to achieve relatively low reaction times, as well as no measurable differences among them due to variations in the amount of information transmitted. The purpose of this study was to investigate this possibility.*

Before presenting method and results it is necessary to make three points:—

(1) The word measurable in the above paragraph is as important as when it was used in a similar context by Helmholtz some hundred odd years ago. It will be gone into more carefully in the discussion; for the moment it may suffice to point out that absence of a measurable effect due to variations in experimental conditions need not by itself mean the absence of effects on underlying mechanisms.

(2) Compatibility is here used in the sense in which it has been proposed by Fitts (Fitts and Seeger, 1953, and for an extended discussion, Deininger and Fitts, 1955). The important points to make are that compatibility is an operationally defined concept, that any given form of it is based on the norm of a given population of subjects, and that for the present the role of innate properties and past experience are not distinguished: to put it quite simply, where there are two display-control arrangements which have been equally practiced by a sample of subjects, the one showing the greater efficiency of performance is held to show the greater compatibility effect for that population. Predictions made with regard to compatibility are thus primarily based on prior observation, and only to a lesser degree to underlying mechanisms except in so far as these apply to known physiological and anatomical limits. To the theoretically minded the results of compatibility studies will, however, inevitably lead back to questions relating to the underlying mechanisms, and lend support for studies within and across age groups.

(3) Now as far as manual responding is concerned it is not entirely easy to produce a situation which allows one the following combination of requisites: the ability to use a reasonable range of input variations, high compatibility, and absence of visual and/or motor search factors. The most highly compatible displays for *manual* responding have in general been those in which number of alternatives are varied by varying the number of lights used as stimuli, and these lights had to be arranged in such a manner as to provide a very direct picture of the positions of the keys to be used. If the lights are widely spaced, problems of visual search enter into the whole set up, if they are closely spaced there are discrimination factors to be considered. Tactual stimulation of the fingers seems to be the answer and it may be of interest to describe the first arrangement tried out by the author and Crossman. The latter had developed a method by which the interruptions of a very low current passed through the hand of an operator could be made to operate a timing sevice. The stimulus, therefore, consisted of a metal pin in circuit with the timer, and the response was to withdraw from the pinprick, the length of time the pin remained in contact with the finger being clearly a reaction time. First attempts with this arrangement seemed promising, but it became quite clear that, delightfully simple as it was, it introduced too many uncontrollable factors, such as the experimenter's pressure and variations in the moistness of the skin of the subject, and was perhaps not entirely suitable for use with other than very devoted subjects. To regularize the position the present arrangement of relays fed with A.C. was developed and though somewhat crude, has proved to be quite satisfactory.

METHOD

Apparatus

This consisted of ten Type 600 relays, with 150 ohms coils, mounted vertically in two sets of five, and spaced in such a way that the five fingers of a subject would rest with tolerable comfort on the armatures of the relays. The relays for the thumbs were in fact not used. The two sets of relays were mounted independently of each other, with a mecanno plate slightly above the level of the relay armatures so that the subject could rest his hands on it. Stimulation was provided by supplying selectively 50-cycle, approx. 30 volts a.c. to the coils of the relays. The subject responded by depressing the relay armature. A make-contact, specially adjusted, signalled the response. (The special adjustment was necessary since surges in the supply might cause a closing of the make contacts themselves. These surges add some variability to the stimulus intensity and are somewhat of a nuisance. It is hoped to overcome them in subsequent equipment.) The experimenter selected the appropriate relay by means of one of ten switches, and had then to initiate the stimulation by the depression of one single common switch. This operation also started a decatron timer, operating in one-hundredth of seconds, and the subject's response, whether right or wrong, terminated the cycle of events. Errors were indicated by the lighting of a single lamp on the experimenter's control panel whenever a wrong relay was depressed. Note that this did not, of course, serve to establish the nature of the error. There was a certain amount of noise from the relays associated with the experimenter's side of the apparatus, but this could provide only a general and not a specific cue. Subsequent experimentation under improved conditions has shown this to have been an apparently unimportant weakness. The subsequently measured lag of the whole system was of the order of 0·04 sec.

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* While this paper was in preparation Mowbray and Rhoades (1959) have shown that extended practice reduced the difference between two-choice and four-choice reaction times to zero. In their task the subject had to respond to visual stimuli by pressing keys.

PROCEDURE

In this kind of experiment the role of finger differences is naturally rather more important than in many others since the fingers have to do the sensing as well as the responding. For this reason two groups of subjects were used. The manner in which they responded is shown below:—

Condition	*Single*	2-*choice*	4-*choice*	8-*choice*
Group I	R. index	R. index and middle	R. all fingers	R. and L. all fingers
Group II	R. index	R. index and L. index	R. index and ring L. index and ring	R. and L. all fingers

Ideally one would, of course, want all possible combinations of fingers for the conditions with lower degrees of choice. Since this was not practicable only the times for the right index fingers were used when calculating mean reaction times.

For each of the three multichoice conditions four separate lists of stimuli were generated without any constraints from a table of random numbers. Subjects attended on four successive days and were given a different list for each condition on each day. During each session they were given the four conditions in four different orders, and the combinations of orders and lists were arranged in four Latin squares. During each session subjects were given 48 single stimuli, 48 2-choice stimuli, 80 4-choice stimuli, and 120 8-choice stimuli. The data to be presented were those obtained on the fourth day. A subsequent comparison of group means for Group I on the third and fourth days confirmed the assumption that performance had become sufficiently stabilised.

During the experiment the experimenter sat facing the subject with a small wooden screen between them to prevent the subject from being able to see the experimenter's selection of stimuli. The experimenter could, however, see the subject's hands. Before each stimulus presentation the experimenter called out "ready." The fore-period cannot be said to have been strictly controlled, but a certain amount of deliberate variability was introduced under all conditions. Ideally a device such as that described by Klemmer (1957) would be desirable to provide controlled fore-period variability.

SCORING

The reaction time was measured as the length of time during which a break contact on a relay remained open. The experimeter initiated this relay and it was held until the holding circuit was broken by the subject's response, whether right or wrong. It should be noted that since the data for one and the same finger only were used to calculate means for all conditions, and since completely random sequences were used, the means per subject are calculated from totals made up of varying numbers of readings. Readings more than 0·05 sec. away from the bulk of the distributions obtained for each subject were excluded from the main calculations after inspection of the distributions had indicated this as a reasonable cutting point. The effect of these exclusions may be seen in the last but one row of entries for each subject in Table I in which the means without exclusions are given (mean (*b*)).

RESULTS

Table I shows values for the right index fingers of the eight subjects used in the experiment, as well as the absolute number of errors made by each of them in each of the four conditions. Subjects A-D were in group I, subjects E-H in group II. The first entry in each block is the mean reaction time, the number in brackets behind it indicates the number of readings from which this mean was calculated (mean (*a*)). The next entry below is the median, followed by the range. The fourth entry is the mean reaction time obtained by retaining readings more than 0·05 sec. away from the bulk of the distribution, the number in brackets showing again the number of readings from which this mean was calculated (mean (*b*)). The last entry in each block is the total number of errors made by each subject in that condition.

Table II shows the mean reaction times for the two groups separately, and combined for the two groups, as well as the error percentages for both groups.

Before discussing the results in these tables in detail it is worth mentioning that in most of the results presented since Hick's paper in 1952 either the data for all separate subjects, or at least the group means, showed progressive and lawful increases of reaction time with information transmitted.

Examination of the mean reaction times (first entry in each block on Table I) shows that all subjects had lower reaction times for the single condition than for any of the choice conditions. It will also be seen that there was only one subject who showed a rank order increase of reaction time with an increase in number of alternatives (subject H). In group I two subjects show an increase between 2 and 4 choice, and two a decrease. The same applies to the subjects in group II. In group I all subjects showed a decrease between 4 choice and 8 choice, and in group II three subjects showed an increase between these two conditions. In group I three subjects had lower reaction times for the 8 choice than for the 2 choice, and in group II two subjects showed the same effect. The group means in Table II reflect these various effects. It is possible that the higher reaction times for the 4 choice condition in group I are indicative of some difficulties in discrimination when all and only the four adjoining fingers were used. On the whole, however, it seems reasonable to say that the overall picture is one in which there is a clear difference between single reaction times and choice times, but there are no systematic differences among the mean times for degrees of choice.

The medians and ranges in Table I are presented to give some indication of the skewness. The second means (*b*) are presented to show the effect of excluding readings more than 0·05 sec. away from the bulk of the distributions.

Concerning errors there also did not seem to be any systematic error pattern and the overall error level was tolerably low. Observations of subjects suggested that in many cases errors had been due to difficulties imposed by the mechanics of separate finger movement, and it is at least suggestive that in two out of three instances the largest incidence of errors are accompanied by those particular subjects' slowest mean times.

TABLE I

SHOWING MEAN REACTION TIMES, MEDIANS, RANGES, AND ABSOLUTE NUMBER OF ERRORS FOR EACH SUBJECT (For details see text)

Group I	*Single*	*2 choice*	*4 choice*	*8 choice*	
S	0·173 (48)	0·208 (21)	0·241 (16)	0·211 (16)	mean (*a*)
A	0·160	0·200	0·240	0·205	median
	0·14–0·27	0·15–0·28	0·17–0·32	0·15–0·27	range
	–	–	–	–	mean (*b*)
	0	2	7	3	errors
S	0·184 (47)	0·201 (29)	0·227 (19)	0·195 (13)	mean (*a*)
B	0·180	0·200	0·220	0·190	median
	0·16–0·27	0·17–0·26	0·17–0·28	0·17–0·23	range
	0·190 (48)	–	–	–	mean (*b*)
	0	2	1	2	errors
S	0·148 (47)	0·235 (25)	0·216 (19)	0·207 (20)	mean (*a*)
C	0·150	0·230	0·210	0·200	median
	0·11–0.18	0·20–0·32	0·18–0·29	0·17–0·30	range
	0·151 (48)	0·250 (26)	0·237 (20)	–	mean (*b*)
	0	0	2	7	errors
S	0·183 (47)	0·262 (24)	0·247 (27)	0·224 (20)	mean (*a*)
D	0·170	0·250	0·230	0·220	median
	0·14–0·30	0·20–0·38	0·20–0·33	0·20–0·29	range
	0·187 (48)	–	–	–	mean (*b*)
	0	1	1	0	errors
Group II					
S	0·186 (45)	0·229 (27)	0·222 (22)	0·231 (16)	mean (*a*)
E	0·180	0·230	0·220	0·235	median
	0·15–0·26	0·17–0·34	0·17–0·26	0·21–0·30	range
	0·200 (48)	–	–	–	mean (*b*)
	0	0	2	0	errors
S	0·201 (47)	0·223 (18)	0·235 (25)	0·206 (13)	mean (*a*)
F	0·200	0·225	0·230	0·200	median
	0·15–0·30	0·18–0·27	0·19–0·33	0·17–0·26	range
	0·207 (48)	0·236 (19)	–	–	mean (*b*)
	0	3	0	0	errors
S	0·197 (46)	0·231 (20)	0·227 (26)	0·230 (19)	mean (*a*)
G	0·200	0·230	0·220	0·220	median
	0·16–0·24	0·17–0·27	0·19–0·29	0·18–0·30	range
	0·207 (48)	0·255 (22)	–	0·237 (20)	mean (*b*)
	0	0	0	0	errors
S	0·181 (47)	0·215 (22)	0·218 (14)	0·234 (19)	mean (*a*)
H	0·180	0·220	0·210	0·230	median
	0·14–0·28	0·14–0·28	0·19–0·27	0·18–0·29	range
	0·193 (48)	0·254 (24)	0·211 (15)	0·248 (20)	mean (*b*)
	0	1	5	8	errors

TABLE II

SHOWING MEAN REACTION TIMES IN SECONDS FOR THE TWO GROUPS SEPARATELY AND COMBINED, AS WELL AS ERROR PERCENTAGES FOR THE COMBINED GROUPS

Mean of means (*a*) Group I	=	0·172	0·227	0·233	0·209
Mean of means (*a*) Group II	=	0·191	0·225	0·226	0·225
Mean of means (*a*) Groups I-II	=	0·182	0·226	0·229	0·217
Percent Errors, Groups I-II	=	0·0%	2·3%	2·8%	2·1%

DISCUSSION

The results of this experiment appear to be clear-cut. If one is prepared to accept the treatment accorded to the data, i.e. in particular, the extraction of the mean times for a single finger for all conditions, it has been shown that there was no systematic effect on choice-time when alternatives were varied among 2, 4, and 8 degrees of choice, but that there was a systematically shorter reaction time in the single reaction situation. This has been achieved in a manual response task requiring relatively little practice, as practice requirements go in such tasks these days: the pretest experience of our subjects was no more than 888 responses per subject. One may say with some confidence that if it were possible to have a somewhat less awkward mode of response the same effects might be achieved with even less practice.

Now there arise two quite separate issues. The first, and at present perhaps most important, is that posed by the difference between single and multichoice. Bricker (1955) had already pointed out the rather intriguing fact that the application of information measures and theory to reaction time data might reopen the question of the meaning of single or simple reaction time since in those cases in which experimenters had been able to fit equations of the form $r.t. = a + b(\log_n)$ [where n is the number of alternatives], the value of the intercept a was usually sufficiently close to the observed simple reaction time to make one consider this as more than a chance event. And one may well argue that the use of that formula might be held to have rather far-reaching implications in the sense that it marks a return to Wundt's concept of the act of choice being something tagged on to the simple reaction time. Hick's (1952) formula does not have this implication, and is in that sense perhaps more in line with the traditional view of the Wuertzburgers. At the same time, the recently published results by Klemmer (1957) show that simple reaction time itself varies with fore-period uncertainty in a linear fashion. Whatever view one likes to take, the time is clearly ripe for a re-examination of the difference between simple and choice reaction situations, and it is to be hoped that in that re-examination the contributions of the last century will not be overlooked.

In that process one should start with a comparison of the distributions of the times obtained for single and multichoice reaction time situations. One hypothesis worth testing is to establish whether the observed differences can be said to be due to real shifts in the distributions or whether they are due to changes in the shapes of the distributions. Yet another approach is along lines suggested by Venables (private communication) in which one tests for the presence of periodicities in reaction time data. Here the hypothesis to be tested would be to establish firstly whether periodicities can be found in both single and multichoice situations, and if so, whether they can be accounted for by the same or different models. Finally it may be worth recalling the controversy about "muscular" versus "sensorial" set in simple reaction time studies (Woodworth, 1938, pp. 306–8). When it was possible to achieve and maintain such sets differentially the former gave faster reaction times than the latter. One might argue on that basis that multi-choice times can never reach the level of simple reaction times since a "sensorial" set is implicit in the multi-choice situation, while a "muscular" set is perhaps the more natural in the simple reaction situation.

The second issue raised concerns the relation between information transmitted and choice time. It should be pointed out that the effect demonstrated in the present experiment, the absence of a measurable effect on reaction time due to varying the number of alternatives, represents one of an infinitely large number of possible linear relationships between time and information transmitted. It must be recalled that in the majority of other relevant experiments, lawful relationships, expressible as functions with varying intercepts and slope constants, have been found. The present finding should not be regarded as contradicting earlier findings, but rather to stimulate the search for models and explanations which will account for all of them. It is hoped to present one such model in a later paper. For the moment it is perhaps wiser to adopt a cautious attitude and consider the possibilities of the present findings being due to an artefact of some sort of another. There are three possible sources of trouble which may be mentioned briefly: (1) Zero effects can be obtained when for some reason or another the general level of responding is slow; bad discriminability is one such reason (for an instance *see* Crossman, 1955, page 192). On the whole one would have thought that this stricture need not apply here since 8 choice times of the order of 0·22 sec. would be accepted as quite fast. (2) Zero effects could also be observed if subjects had run up against some sort of response limit. In the present case this is ruled out to some extent due to the fact that subjects were able to respond faster in the single choice situation; but the objection might still hold if it were to be shown eventually that there is a qualitative difference between single and multichoice situations. (3) Zero effects could also be due to the fact that subjects, who have had experiences of all conditions, adopt the level of the most complex condition for all others. Again, this is partially ruled out in the present case by the same arguments as those given in (2) above. But ideally, one would like to carry out some of these experiments with large groups of subjects, each group operating under one condition only. An alternative to this may be to provide rather specific immediate knowledge of results, such as the giving of a click indicating that a response time fell below a given target value. I used this method (Leonard, 1958), but after several months of practice on a visual motor task still showed a difference between 3 and 6 choice though that difference was small.

The three sources of artefact mentioned are perhaps not exhaustive. I have gone into some detail over them to emphasize a point made in the introduction about the necessary caution with which one has to approach the absence of an effect as demonstrated in the present experiment.

In subsequent work it is hoped to be able to consider some of the difficulties raised here. At the same time I consider the results obtained in this experiment to be reasonable and in line with earlier work. Because of this it may be more profitable to concentrate on the re-examination of the difference between simple and choice reaction times.

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CHANGES IN BEHAVIOUR ASSOCIATED WITH A NERVOUS SYSTEM POISON (D.D.T.)

BY

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The main purpose of the investigation was to show that behaviour measures can be used to investigate the effects of those toxic drugs which produce "biochemical lesions" in the nervous system although the nature of the lesions still remains undetermined. The advantages of this approach are twofold. First, a psychological study may help to uncover the initial effects of the drug, and thus provide evidence which may lead to the ultimate understanding of the action of the drug. Second, and of value from the practical point of view, such a method may be used to detect toxicity.

In this study D.D.T. was used. Two experiments were performed on one control and four experimental groups of albino rats. Problem solving behaviour, speed and pattern of locomotion, and reaction to stress involving visual stimuli were observed.

Problem solving behaviour was found to be unaffected by the drug; no changes were found in speed of locomotion, but pattern of locomotion revealed that "ataxia" was one of the initial effects of D.D.T. poisioning. The experimental animals were found to be generally less reactive to "stress"; "hyper-irritability" reported in previous studies being explicable in terms of exaggerated motor responses.

The results obtained on "ataxia" showed that the procedure adopted here could be used to detect chronic D.D.T. toxicity in rats.

INTRODUCTION

The study of changes in behaviour following "biochemical lesions" (Peters, 1948) produced in the Central Nervous System (C.N.S.) is a technique developed comparatively recently. It has been used to study the relation existing between physiological and psychological factors (e.g. Russell, 1954; Watson, 1955; Knopfelmacher *et al.*, 1956), and has two advantages. It is independent of the structural arrangement of the various areas in the brain, thus it overcomes the difficulties introduced by anatomical individual differences. The biochemical changes can in general be maintained for a sufficient length of time to allow experiments on behaviour to be carried out, and in this way it avoids the difficulties introduced by "post-operative" recovery.

Biochemical lesions involve interference with metabolic events. Administration of toxic drugs is an effective and easy method for producing these desired changes. However, if the function in behaviour of a certain structure in the C.N.S. is under investigation, then this technique can only be used fruitfully when the specific nature of the biochemical lesions produced is known to the experimenter. That is to say unless the action of the drug is known it cannot be adequately used as an agent for producing biochemical lesions. In this way a restriction is imposed on the technique.

It is possible to show relationships between certain neuro-physiological processes and behaviour by interfering with the metabolism of nervous tissues. We should, therefore, be able to suggest the possible mode of action of a nervous system poison from a study of associated behaviour changes. This method of approach provides a way of dealing with toxic drugs which produce biochemical lesions in the nervous system when the nature of the lesions still remains undetermined. Systematic observation of behaviour deviations from normal could be used to infer or deduce the nature of the disturbances created in the nervous system by unknown biochemical lesions (due to the action of the toxic drug).

Thus two important results can be derived from studying behaviour changes following drug administration: (*a*) psychological methods may help to uncover the initial effects of the drug, and (*b*) such methods may be used to detect toxicity.

In the present research dichlorodiphenyltrichloroethane, more commonly known as D.D.T., was used because although the symptoms of poisoning showed that the nervous system was involved (Cameron and Burgess, 1945; Philips and Gilman, 1946; Bing and McNamara, 1946), the exact biochemical action of the drug as well as the nature of the lesions produced still remain unknown. Rats were used as subjects, the experiments being performed on a control group to which no D.D.T. had been given, and four experimental groups to which various doses of the drug were administered. The study covered systematically several aspects of behaviour. Physiological research on the effect of D.D.T. revealed no evidence of involvement of the cerebral cortex, nevertheless, the possibility of D.D.T. affecting normal cerebral functioning was investigated. The effect of the drug on more simple forms of behaviour, namely the speed and pattern of locomotion, was also considered (the latter aspect of behaviour appeared to be relevant in the course of experimentation). Finally, it has been reported (e.g. Cameron and Burgess, 1945) that in the early stages D.D.T. toxicity, animals suffering from poisoning by the drug were in a state of "hyper-irritability" or showing great sensitivity to stimuli. The possibility that D.D.T. might produce greater sensitivity generally to external stimulation was therefore studied.

Experimental Design and Procedure

Two experiments have been conducted. The first was designed to study the effect of D.D.T. on problem solving behaviour, speed and pattern of locomotion. The second experiment was designed to investigate the effect of the drug on the reaction to "stress" (Russell, 1952).

The situation used to study the effcts of D.D.T. on problem solving behaviour was the closed field test (Hebb and Williams, 1946) as modified by Rabinovitch and Rosvold (1951). In Phase 1 all the groups went through preliminary training. Problem solving behaviour was studied in Phase 2. Finally speed and pattern of locomotion were observed in Phases 3 and 4 respectively.

The open field (Hall, 1934) test was used to study reaction to stress. Stress is defined here in terms of a conflict situation of the "approach-avoidance" type (Miller, 1944). The positive stimulus used was food presented to the hungry rats. A very bright light was the negative stimulus which the rats tried to avoid.

Method of administration of the drug

The drug was administered orally by mixing it with the diet (M.R.C. 41, Bruce and Parkes, 1949) on which the animals were fed. Arachis oil was used as solvent for the D.D.T. Group C (the control group) was fed on diet 41 to which was mixed 1 millilitre of Arachis oil per 75 grm. of food. The diet used for the experimental groups E_1, E_2, E_3 and E_4 contained D.D.T. dissolved in the same proportion of Arachis oil as the proportion used in Group C diet. The doses of D.D.T. used were the following: 100, 200. 400 and 600 parts per million (p.p.m.) of D.D.T. respectively. Symptoms of acute D.D.T. poisoning appear at the 800 p.p.m. level.

The subjects were fed 1 hour every 24 hours with their respective diet. Three animals of the same group were kept in a cage, and food was introduced in the cage and then withdrawn at the end of the hour. An *ad libitum* supply of water was available to the subjects in their home cages. During experimentation after the third animal in a cage had completed its session for the day, the group was fed. The food intake for each group was checked daily. All the animals were weighed five times a week after all the feeding had been completed.

Problem solving behaviour, speed and pattern locomotion

Fifteen male albino rats were obtained from the colony of the Toxicology Unit of the M.R.C. Litter mates were assigned at random to one of the five groups used for experimentation. No rats belonging to the same litter were included in the same group. The animals were 122–126 days old when the groups were formed.

The apparatus consisted of "the field"; a square box with a starting box at one corner and a food box at the opposite corner of the same diagonal. A number of different sized barriers were used to make up the problems. The barriers were arranged in different patterns so as to block in various ways the path between the starting box and the food box leaving one free passage, and introducing blind alleys or "error zones." Retracing doors prevented the animals from returning to the starting box once they had entered the "field," and from leaving the food box once they had reached the latter. Two sets of problem situation standardized by Rabinovitch and Rosvold were used. The first set consisted of six practice problems, which were used in rotation during the preliminary training. The second set consisted of twelve problems which were used during Phase 2.

Twenty-one days after the groups had been on their respective diets, Phase 1 was begun. Three animals of the same group were released together from the starting box, and were allowed to explore the field or feed for 15 min., after which they were withdrawn from the apparatus. The food given to the animals in the apparatus was the same as their experimental diet. After five days of group training, the procedure was changed in individual training. Now each subject in turn was given nine trials a day, and was allowed 15 sec. in the food box after each trial. When all the animals had completed nine trials in 60 sec. or less on two consecutive days the preliminary training was concluded.

In Phase 2 the animals were exposed on successive days to one of twelve problems constituting the second set. Each rat was given seven trials on every problem. Errors representing the number of times an animal entered the error zones, and the time in seconds taken for each trial were recorded.

The speed of locomotion was investigated on the day following the completion of Phase 2. The barriers were arranged to form a straightaway for Phase 3 and 4. Data on the speed of running of the animals was obtained by recording the time taken on each of the seven trials over three consecutive days.

The procedure adopted for observing the speed of locomotion was repeated in Phase 4, but the animals were given five trials instead of seven. In the first three trials the animals were made to run the straightaway only the time being recorded. In the fourth and fifth trials the hind paws of the animals were smeared with vaseline and they then ran the straightaway over a fixed strip of filter paper. Pulverized charcoal was sprinkled over each record, and the foot-prints thus obtained were made permanent by spraying charcoal fixative over the record. Scoring was as follows: the line joining the print of the right paw to that of the left paw was taken as the hypothenuse along which a right-angle triangle was constructed. The sides of each triangle were drawn parallel to the length and width of the filter paper strip used as record sheet, that parallel to the width being taken as the base of the triangle. The base and height of the triangle were measured in millimetres and the tangent of the angle at the apex of the triangle—the "gait angle"—was calculated. Figure 1 shows a specimen of the footprints obtained from one of the subjects, and the right-angled triangles constructed in order to score the record.

Reaction to stress

Thirty male albino rats from the same colony as those used in Experiment 1 served as subjects. The grouping techniques and the age at which the groups were formed were the same as in the previous experiment.

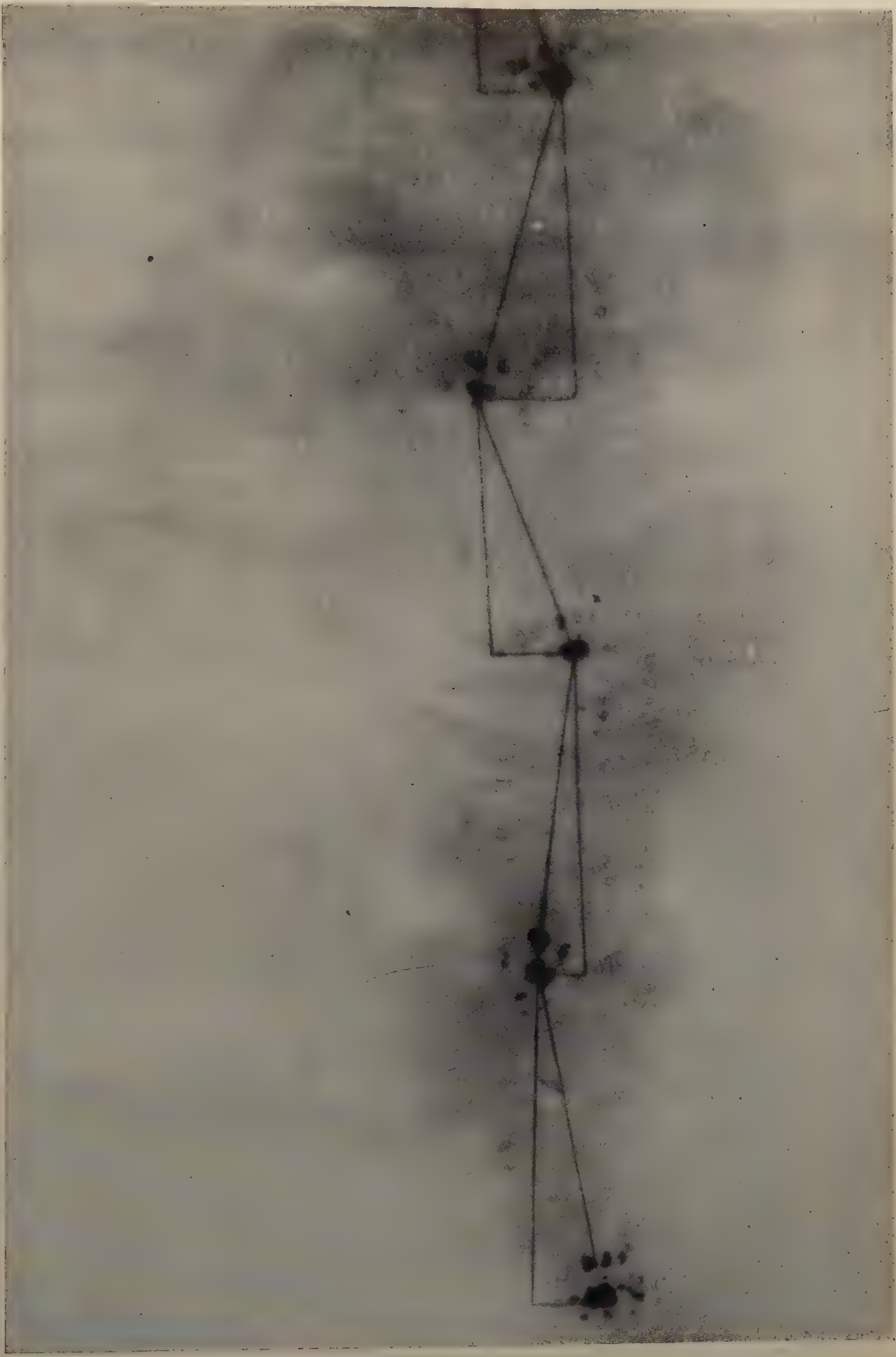
The apparatus consisted of a floor area zoned off by a circular enclosure, 72 in. in diameter and 18 in. in height. The floor area was divided into segments by radii and concentric circles in order to allow observation of the subjects movement. A very bright light, 200-watt bulb, was suspended 18 in. above the centre of the enclosure, and illuminated the zoned-off area only. A food pot was placed immediately below the suspended light, and the food presented to the animals was the same as the diet on which they were maintained.

Twenty-one days after the groups had been put on their various diets the open field test was started. The subjects were given one trial a day, the length of the trial being 2 min. The test was carried out for twelve consecutive days, and during each trial observations were made of seven different types of responses.

RESULTS

In both experiments the measures taken were dependent on the levels of hunger drive among the various groups. Consequently the food intake for each group was checked daily in case D.D.T. when added to the diet caused "food shyness" or

FIGURE I



Record for studying the pattern of locomotion

"anorexia" in the subjects. In neither experiment was there a significant trend towards a reduction in food intake during experimentation. These results are in agreement with previous findings (Fitzhugh and Nelson, 1947; Lang *et al.*, 1950).

Effects on problem solving.

The number of errors made by the animals on each problem and the total number of errors made over the twelve provided measures of the subjects' behaviour on the serial problem solving task.

Table I gives the medians of the total error scores and the range of each group.

TABLE I

SERIAL PROBLEM SOLVING—TOTAL ERROR SCORES

*Group**	*No. of Subjects*	*Median*	*Range*
C	3	186	93–223
E_1	3	183	92–191
E_2	3	125	98–146
E_3	3	134	100–209
E_4	3	182	40–201

*None of the differences between the groups is significant.

Using the Fisher-Yates test of significance, comparisons of the groups for scores obtained on each problem, as well as the total number of errors made on the twelve problems revealed no significant differences.

Effects on speed and pattern of locomotion

Analysis of the data obtained on the straightaway showed that D.D.T. had no effect on the speed of running of the animals.

Two records were obtained in studying the pattern of locomotion for each animal (the method for calculating the tangents of the gait angles was described above when the procedure for studying the pattern of locomotion was given). The score for each animal was taken to be the mean tangents of the gait angles obtained from the two records. Table II gives the mean, median, and range for each group.

TABLE II

PATTERN OF LOCOMOTION—MEAN TANGENT OF GAIT ANGLES

			*Group**		
	C	E_1	E_2	E_3	E_4
MEAN ..	0·746	0·407	0·722	0·821	0·895
MEDIAN ..	0·689	0·401	0·712	0·782	0·875
RANGE ..	0·484–1·065	0·301–0·519	0·580–0·810	0·636–1·045	0·521–1·289

* The trend for increasing mean tangent scores obtained for the experimental groups is significant at the 0·007 level of confidence.

The examination of the value of the mean tangents obtained for the various groups showed that these scores increased as the concentration of D.D.T. administered was increased. The trend for Groups E_1, E_2, E_3 and E_4 was found significant ($p = 0·007$) when applying a test for trend (Jonchheere, 1954). The trend when including group C is also significant ($p = 0·05$).

Effect on reaction to stress

Seven types of response were recorded to study the effect of D.D.T. on reaction to stress: amount of locomotion, frequency of rearing, preening, urination, defecation, and squeaking, and the number of trials during which the subject fed.

Table III presents the median scores and their ranges for the various measures of reaction to stress among the various groups.

TABLE III

REACTION TO STRESS—MEDIAN SCORES

	Group				
	C	E_1	E_2	E_3	E_4
NO. OF SUBJECTS	6	6	6	6	6
AMBULATION:					
Median	50·8	54·3	43·5	53·5	66·0
Range	25·5–85·5	40–94	11·5–81·5	88–71	35·5–90
REARING:					
Median	63·5	53·0	72·0	55·5	44·5
Range	36–116	38–89	47–216	33–109	25–81
PREENING					
Median	21·5	16·5	10·5	4·5	7·5
Range	10–50	4–39	0–52	2–34	5–44
URINATION[1]:					
Median	1·5	0·0	0·0	0·0	0·5
Range	0–4	0–1	0–4	0–1	0–4
DEFECATION:					
Median	1·5	0·0	1·0	0·0	0·5
Range	0–5	0–1	0–4	0–1	0–8
SQUEAKING:					
Median	0·5	1·0	1·0	0·5	2·5
Range	0–3	0–7	0–4	0–2	0–6
FEEDING:					
Median	8·0	8·5	7·5	8·5	7·0
Range	0–9	7–10	0–12	0–10	0–11

[1] The difference between Group C and the combined experimental groups is significant at the 0·02 level of confidence.

Since the differences between Groups E_1, E_2, E_3 and E_4 were found to be not significant on the various measures these groups were combined for analysis purposes. Comparisons between Group C and the combined experimental groups showed no significant differences except on the urination scores. The difference on this latter measure was found to be significant ($p = 0·02$) but in the opposite direction to that predicted on the basis of "hyper-irritability." The interpretation of this result would be impossible if there was not some supporting evidence on the defecation scores, and on the rearing and preening scores. On these three measures although the differences among the groups are not significant, the experimental groups scored consistently less than Group C. High rates of defecation and urination on the open field test were claimed to be indicative of "emotionality" in rats (Hall, 1934). On the other hand, rearing and preening referred to as "displacement activities" were said to be characteristic responses to conflict situations. D.D.T. seemed to produce a reduction in the occurrence of these four types of responses in a stress situation.

Discussion

The results showed four basic points:—

(1) Problem solving was unaffected by the drug.

(2) The speed of locomotion was unaffected by D.D.T.

(3) Significant alternation in the pattern of locomotion had taken place.

(4) Finally, compared to the control group, the experimental groups showed less reaction to stress, although this was significant for only one type of response.

That no change was found in problem solving behaviour with D.D.T. administration was compatible with the conclusions drawn from physiological studies by other research workers. There was general agreement that the drug had no effect on the cerebral cortex.

It was claimed by Carey *et al.* (1946), that degeneration of motor end plates and atrophy of certain muscle fibres did take place with high doses of D.D.T. administration. These findings were not reflected in the results obtained on the speed of locomotion. No significant differences in speed of running were obtained among the groups.

The results showed that one of the initial effects of D.D.T. was to produce "ataxia." "Ataxia" has been associated with two different kinds of physiological disturbances, (*a*) lesions in the cerebellum, and (*b*) peripheral degenerations particularly of sensory nerves as for instance in the condition known as Tabes dorsalis. Therefore these results supported two of the many hypotheses associated with the action of D.D.T. Hypothesis (*a*) maintains that the centre of action of D.D.T. is the cerebellum. This assumption was put forward because of striking similarities existing between symptoms due to cerebellar lesion and symptoms following acute D.D.T. poisoning. Some evidence from E.E.G. recordings following D.D.T. administration substantiate this hypothesis. Significant changes in rhythm were observed from leads inserted in the cerebellum (Crescitelli and Gilman, 1946). However, these observations were made with curarized animals which makes a complete interpretation of these results rather difficult. Hypothesis (*b*) maintains that the nervous symptoms following D.D.T. poisoning are primarily due to peripheral degeneration. Here on the one hand motor degeneration was believed to be the essential feature in the action of D.D.T. (Yeager and Munson, 1945; Carey *et al.*, 1946), and on the other D.D.T. was believed to act directly on the peripheral sensory organs as Roeder and Weiant (1948) had shown occurred in insects. Thus, although ataxia was found to be one of the initial effects of D.D.T., the nature of this effect has still to be examined.

From the study on the pattern of locomotion of rats suffering from D.D.T. chronic toxicity a point of further interest arose. It was noted that Group E_1 had very narrow tangents of gait angles. The mean or median for the group was much below the mean or median of Group C. This together with the examination of the records suggested a twofold action of D.D.T. on the pattern of locomotion. First the drug at 100 p.p.m. level produced an exaggeration in the stretch reflex of the animal. Second, with the 200 p.p.m. dose and over there was a broadening of the gait and a shortening of the steps. Exaggeration in stretch was reported by Bing and McNamara (1946) with dogs to whom a comparatively low dose of D.D.T. was administered. Roy (1954) on the other hand using as "preparation" the sensory nerve of the appendage of the cockroach observed two different patterns in the recordings of nerve impulses following D.D.T. administration. With low D.D.T. doses a great increase in the frequency of the impulses was picked up. With higher doses of the drug these bursts were interrupted by periods during which impulses stopped travelling. From the practical point of view the results on the pattern of locomotion suggested that chronic D.D.T. poisoning in rats can be detected by studying this type of behaviour using the procedure adopted here.

Finally, the animals with chronic D.D.T. toxicity were found to be less reactive to stress, and this appears to be in contradiction to physiological studies where animals suffering from D.D.T. poisoning have been described as being generally hypersensitive to stimuli. A suggestion can be put forward to explain this apparent contradiction. The effect of D.D.T. may produce an exaggeration in motor responses of the subject to which the drug is administerred. That means that the animal's responses to stimuli are exaggerated, but its sensitivity to the stimuli as such may remain unaltered by the drug.

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THE PROCESSING OF POSITIVE AND NEGATIVE INFORMATION

BY

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An affirmative statement which is known to be false and the complementary negative statement which is known to be true, provide the same information, i.e. that something is not the case. Similarly, an affirmative statement which is known to be true and the complementary negative statement which is known to be false, both imply that something is the case. (If P is false, not-P is true and if P is true, not-P is false.) Hence there ars four kinds of statement ("conditions"): true affirmatives, false affirmatives, true negativee and false negatives, but only two kinds of information: positive and negative.

This experiment investigates the times taken to process information presented in these ways. The task was to select two alternative words which would make affirmative or negative conjunctive statements agree or conflict with given situations. The four conditions were presented six times in different serial orders, so that each occurred once in every block of four trials. The mean response times were: true affirmatives 8·99 sec., false affirmatives 11·09 sec., true negatives 12·58 sec., false negatives 15·17 sec. This order was the same at each of the six presentations of the conditions, the differences being significant at the 0·001 level in each case. There was a pronounced decline in errors (without knowledge of results) for three of the conditions. These results are discussed in relation to (i) the assumption of a positive set, established through a long learning process; (ii) the inferential nature of negative information in relation to experience; and (iii) the possible emotional effects of negative terms.

INTRODUCTION

A statement of fact can be affirmative or negative and (in two-valued logic) it can be true or false. An affirmative statement, which is known to be false, and the complementary negative statement, which is known to be true, provide the same information—that something is not the case. (For example, if I establish that the statement, "There is an essay on this desk," is false, then I know that the statement, "There is not an essay on this desk," is true.) Similarly, to know that an affirmative statement is true is to know that a complementary negative one is false.

There are accordingly four classes of statement, which will be referred to collectively as the "conditions": (1) true affirmatives, (2) false affirmatives, (3) true negatives, and (4) false negatives. But there are only two kinds of "information" which can be derived from them: positive information and negative information. The information is the knowledge about the state of affairs, or "situation" which can be inferred by interpreting a statement and its truth value together. It is positive when it allows a valid inference about what the situation is and negative when it allows a valid inference only about what the situation is not. Table I exemplifies the relations which hold between situation, statement, truth value and information.

These relations are, of course, purely formal or logical. The psychological problem is to observe how these conditions can be processed or handled, and whether the information which can theoretically be derived from them is of any assistance. There are three sorts of experiment which might elucidate the problem. In the first place, an affirmative or negative statement can be selected or constructed so that it is true or false. For example, given the situation x and instructions that an affirmative statement is to be false in relation to it, the statement, "There is y" can be selected, rather than the alternative "There is *x*." Secondly, a situation can be selected so that an affirmative or negative statement is true or false. This is simply the converse of selecting the statement. Finally, an affirmative or negative statement can be verified as true or false, e.g. given the situation *x* and the statement, "there is not *y*," the word "true" can be selected, rather than "false."

TABLE I

RELATIONS BETWEEN SITUATION, STATEMENT, TRUTH VALUE AND INFORMATION

Condition	*Situation*	*Statement*	*Truth value*	*Information*
1. TRUE AFFIRMATIVE	*x*	"There is *x*"	TRUE	POSITIVE ("*x* is true")
2. FALSE AFFIRMATIVE	*x*	"There is *y*"	FALSE	NEGATIVE ("*y* is false")
3. TRUE NEGATIVE . .	*x*	"There is not *y*"	TRUE	NEGATIVE ("not *y* is true")
4. FALSE NEGATIVE . .	*x*	"There is not *x*"	FALSE	POSITIVE ("not *x* is false")

Note.—For convenience the situation is here held constant and the value in the statement varied.

TABLE II

SERIAL ORDERS OF CONDITIONS

Subjects	*First presentation of each condition*	*Second presentation of each condition*	*Third presentation of each condition*	*Fourth presentation of each condition*	*Fifth presentation of each condition*	*Sixth presentation of each condition*
(1)	1 2 3 4	2 1 4 3	2 4 1 3	2 1 3 4	1 2 4 3	1 4 2 3
(2)	2 1 3 4	1 2 4 3	1 4 2 3	1 2 3 4	2 1 4 3	2 4 1 3
(3)	3 1 2 4	1 3 4 2	1 4 3 2	1 3 2 4	3 1 4 2	3 4 1 2
(4)	4 1 2 3	1 4 3 2	1 3 4 2	1 4 2 3	4 1 3 2	4 3 1 2
(5)	1 2 4 3	2 1 3 4	2 3 1 4	2 1 4 3	1 2 3 4	1 3 2 4
(6)	2 1 4 3	1 2 3 4	1 3 2 4	1 2 4 3	2 1 3 4	2 3 1 4
(7)	3 1 4 2	1 3 2 4	1 2 3 4	1 3 4 2	3 1 2 4	3 2 1 4
(8)	4 1 3 2	1 4 2 3	1 2 4 3	1 4 3 2	4 1 2 3	4 2 1 3
(9)	1 3 2 4	3 1 4 2	3 4 1 2	3 1 2 4	1 3 4 2	1 4 3 2
(10)	2 3 1 4	3 2 4 1	3 4 2 1	3 2 1 4	2 3 4 1	2 4 3 1
(11)	3 2 1 4	2 3 4 1	2 4 3 1	2 3 1 4	3 2 4 1	3 4 2 1
(12)	4 2 3 1	2 4 1 3	2 1 4 3	2 4 3 1	4 2 1 3	4 1 2 3
(13)	1 3 4 2	3 1 2 4	3 2 1 4	3 1 4 2	1 3 2 4	1 2 3 4
(14)	2 3 4 1	3 2 1 4	3 1 2 4	3 2 4 1	2 3 1 4	2 1 3 4
(15)	3 2 4 1	2 3 1 4	2 1 3 4	2 3 4 1	3 2 1 4	3 1 2 4
(16)	4 2 1 3	2 4 3 1	2 3 4 1	2 4 1 3	4 2 3 1	4 3 2 1
(17)	1 4 2 3	4 1 3 2	4 3 1 2	4 1 2 3	1 4 3 2	1 3 4 2
(18)	2 4 1 3	4 2 3 1	4 3 2 1	4 2 1 3	2 4 3 1	2 3 4 1
(19)	3 4 2 1	4 3 1 2	4 1 3 2	4 3 2 1	3 4 1 2	3 1 4 2
(20)	4 3 1 2	3 4 2 1	3 2 4 1	3 4 1 2	4 3 2 1	4 2 3 1
(21)	1 4 3 2	4 1 2 3	4 2 1 3	4 1 3 2	1 4 2 3	1 2 4 3
(22)	2 4 3 1	4 2 1 3	4 1 2 3	4 2 3 1	2 4 1 3	2 1 4 3
(23)	3 4 1 2	4 3 2 1	4 2 3 1	4 3 1 2	3 4 2 1	3 2 4 1
(24)	4 3 2 1	3 4 1 2	3 1 4 2	3 4 2 1	4 3 1 2	4 1 3 2

Experiment

Aim

To determine whether there are any significant differences in the time taken to identify the following conditions: (1) true affirmative statements, (2) false affirmative statements, (3) true negative statements, and (4) false negative statements.

Design

Presentation of each condition (with alternative answers in the statements) six times in a pre-determined serial order. There are thus 24 trials (each trial using a different situation), the order of the four conditions being arranged so that no accurate prediction can be made about what condition will appear at any one trial. Each subject is assigned to one of 24 different permutations of the serial order (Table II, the numbers 1, 2, 3, 4 refer to the four conditions).

It will be noted that each condition occurs once in every block of four trials, enabling comparisons to be made between successive presentations, and that, for the 24 subjects taken as a whole, each condition occurs six times on every trial. The serial order also allows the transfer effect of any immediately preceding condition (and pairs of immediately preceding conditions) on any other condition to be determined.

Subjects

Twenty-four psychology undergraduates of whom 15 were men and 9 women, their ages ranged from 16 to 25.

Materials

(1) Twenty-four different situations were constructed, each of which consisted of a square (13·5 × 13·5 cm.) divided into four numbered quadrants. Each quadrant always contained a single different coloured star in its far corner. The number in the quadrants remained the same, but the stars, coloured green, red, yellow and black, were varied between quadrants so that no situation was ever identical with another. The arrangement was balanced so that each coloured star appeared in each quadrant six times.

(2) Two of these 24 series of situations were reproduced. Above one series the following instructions were typed (i.e. instruction for "truth"):

"Adjust the statement so that it agrees with the situation."

Above the other series of situations the following was typed (i.e. instructions for "falsity"):

"Adjust the statement so that it conflicts with the situation."

(3) Twenty-four statements of a two-choice kind, each of which was appropriate to two quadrants in every situation, were constructed. Two such series of these statements were reproduced. One series (i.e. affirmatives) was of the following form:

"There is both $\begin{matrix}\text{Yellow}\\ \text{Green}\end{matrix}$ in 4 AND $\begin{matrix}\text{Red}\\ \text{Black}\end{matrix}$ in 3."

The other series (i.e. negatives) was of the following form:

"There is NOT both $\begin{matrix}\text{Yellow}\\ \text{Green}\end{matrix}$ in 4 AND $\begin{matrix}\text{Red}\\ \text{Black}\end{matrix}$ in 3."

One of the alternatives in both components always agreed with the situation and the other always conflicted with it.

These statements were balanced so that the quadrants and the colours mentioned in them appeared as often in the first as in the second component of the statement. The colour names appeared as often in the upper as in the lower positions and the position of "correct" choices was also varied systematically. These two series of 24 statements were duplicated so that any one could be attached directly underneath a situation.

Thus the two kinds of instruction (for truth or falsity) and the two kinds of statement (affirmative or negative) can be combined to produce the four conditions under investigation. (Fig. 1.)

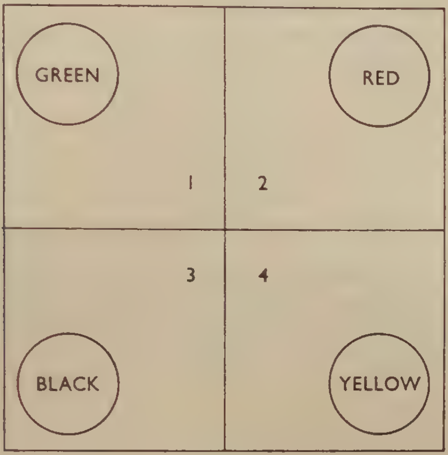
FIGURE 1

EXAMPLE OF MATERIAL

INSTRUCTIONS

Adjust the statement so that it $\left\{\begin{array}{l}\text{agrees}\\ \text{conflicts}\end{array}\right\}$ with the situation.

SITUATION



STATEMENT

"There is (NOT) both $\begin{array}{l}\text{yellow}\\ \text{green}\end{array}$ in 4 AND $\begin{array}{l}\text{red}\\ \text{black}\end{array}$ in 3"

Method

An order of presentation for the 24 situations was worked out which minimized, as far as possible, any serial or position effects. This order was held constant for all subjects. Hence, at any particular trial, from one to 24, every subject is seeing the same situation but is not necessarily operating with it in the same way. The way in which he is instructed to operate with it depends on the condition which holds for him at that particular trial.

The dependent variables were the time taken to select the alternatives in the statements and the errors made.

It will be noticed that logically there is a differential probability for obtaining the correct solution when the information is positive and when it is negative. For the true affirmative and the false negative conditions, i.e. for positive information, there is only one correct solution, hence its probability is 0·25. But for the false affirmative and true negative conditions, i.e. for negative information, there are three correct solutions, hence their probability is 0·75. This differential probability is a necessary consequence of using a conjunctive statement. Since it is sufficient to negate such a statement by making either of its components false, it is necessary to make the form of the true negative statement equivalent logically to that which might be inferred from the instructions for the false affirmative condition; in other words, to express it in the form, "there is not both . . . and . . . ," rather than in the form, "there is neither . . . nor . . . ."

Procedure

Subjects were selected at random (as far as possible) and assigned to one of the 24 permutations of the serial order. They were taken individually, the appropriate conditions having been previously arranged by the Experimenter. The following typed instructions were then presented to them:—

General instructions

This is an experiment to see how well you can fit descriptions to the things which are described.

Three kinds of information will be presented to you at each trial:

(*a*) A "situation" consisting of a square divided into four numbered quadrants with a single coloured star in each.

(*b*) Instructions which will appear above the situation and which will tell you what to do. *These instructions must be read carefully at each trial* as they will not always be the same.

(*c*) A possible description of the situation in the form of a statement which has to be adjusted in accordance with the instructions.

At each trial you must read the instructions and the whole statement in front of you and then make a selection from the alternatives given in that statement (indicating your choice by *encircling*) in accordance with the instructions. For instance, the statement might have the following form:

Mauve Brown
". . . . both in 2 AND in 3."
Red Blue

(The colour words refer to the stars, one in each quadrant, and the numbers to the specific quadrants.)

Your task is to put a circle round any colour word you think is correct but you must never put one round both alternatives named in the same quadrant. In other words, you must never encircle both Mauve *and* Red and/or both Brown *and* Blue. Your choice lies between Mauve and Red and between Brown and Blue.

To summarize: there are three things which you have to examine each time:

(*a*) The situation.
(*b*) The instructions above it.
(*c*) The statement which belongs to the situation.

And your task is to mark the statement in accordance with the instructions above the situation.

You must be careful not to make mistakes but you must also work as fast as is compatible with accuracy. If you do make mistakes, you will increase the number of trials which you have to perform. (This "threat" was, of course, not carried out, but was intended to stress the importance of accuracy.)

Have you any questions?

When they had read this, the Experimenter ran over it again to ensure that they knew what was expected of them. Subjects were told that they would be asked to close their eyes before each trial, that the material would then be placed in front of them on the desk, and that when the Experimenter said "Start," they were to open their eyes and carry out the task without comment. They were told that they could ask questions (without guarantee of answer) in between trials.

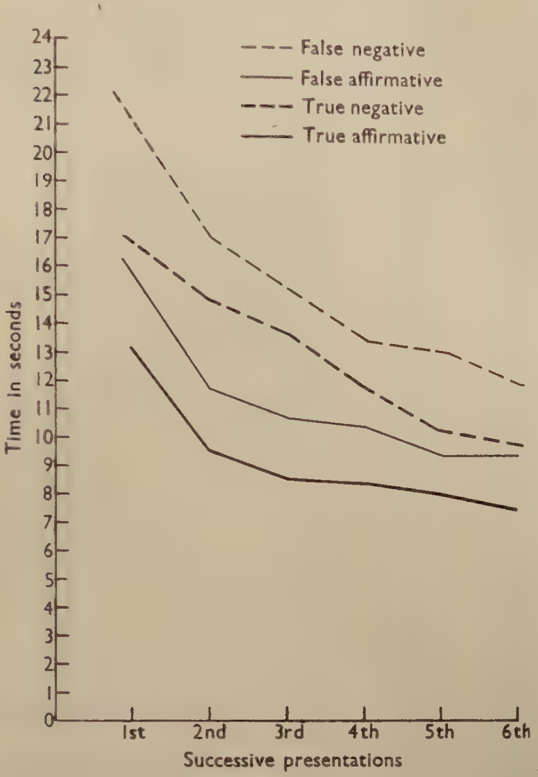
The 24 situations, with their attached statements, were then presented one at a time with a few seconds intervening in between trials. On saying "Start," the Experimenter simultaneously started a stop-watch, stopping it when the subject had lifted his pencil from the paper, having made a selection from the second component in the statement. The material was then removed by the Experimenter and replaced by the material for the next trial. No knowledge of results was given and subjects did not know how many trials to expect. On completion of the last trial, subjects were asked for their introspections. They were then shown examples of the four conditions and asked to order them from one to four according to their assessment of their relative difficulty. They were also asked whether they noticed any logical equivalences between conditions (a question which was not understood and could not be explained without giving the answer). Finally, they were asked to demonstrate exactly what they did as each condition was presented.

Results

1. *Differences between conditions*

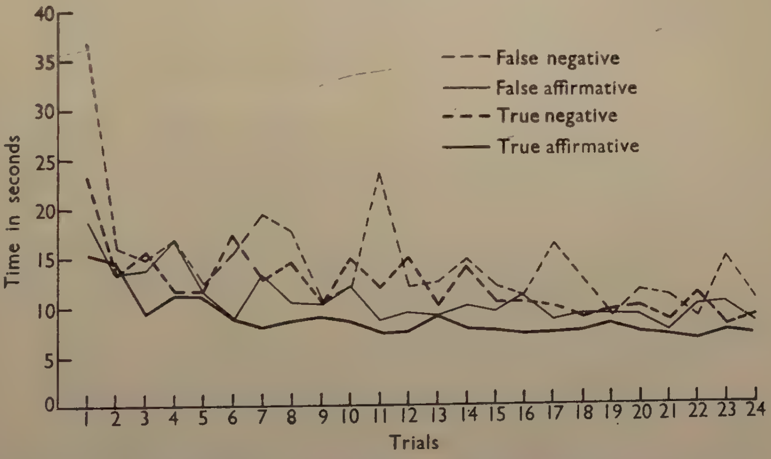
Figure 2 shows the curves of the mean response times to the four conditions on their six presentations, i.e. from each of the six blocks of four trials ($n = 24$). Figure 3 shows the curves of the mean response times for every trial (from one to 24), and Table III shows the corresponding figures ($n = 6$).

FIGURE 2



Mean response times for the conditions at each successive presentation (N = 24).

FIGURE 3



Mean response times for the conditions at every trial (N = 6).

TABLE III

Mean Response Times (in secs.) for each Condition on every Trial ($N = 6$)

Trials	*True affirmative*	*False affirmative*	*True negative*	*False negative*
1	15·92	19·62	24·67	37·87
2	14·96	13·67	13·71	16·54
3	9·33	14·04	16·29	15·12
4	11·42	16·92	12·12	17·20
5	11·33	12·00	11·96	12·79
6	9·08	9·08	17·87	15·96
7	8·04	13·83	13·21	19·95
8	8·83	10·75	15·04	18·25
9	9·12	10·54	10·79	10·96
10	8·79	12·58	15·25	12·87
11	7·54	9·04	12·21	24·16
12	7·75	9·79	15·50	12·21
13	9·46	9·50	10·21	12·87
14	8·00	10·37	14·50	16·12
15	8·04	9·71	10·71	12·42
16	7·54	11·21	10·83	11·29
17	7·67	8·83	10·25	16·87
18	7·71	9·25	9·12	12·92
19	8·46	9·37	9·71	9·33
20	7·67	9·12	10·37	11·83
21	7·21	7·62	8·87	11·37
22	6·83	10·21	11·50	9·00
23	7·75	10·54	8·17	15·25
24	7·37	8·46	9·17	10·92
Mean ..	8·99	11·09	12·58	15·17

The individual response times were transformed into log. times and one-way Analyses of Variance were computed for each of the last five blocks of four trials in the series, i.e. for the second, third, fourth, fifth and sixth presentations of the conditions. No analysis was computed on the first block of four trials because the first presentation of each condition was treated as practice. The differences between conditions on their first presentation are, in any case, highly significant, but it was assumed that they are due primarily to initial comprehension of the words in instructions and statements.

The results of these Analyses (Table IV) show highly significant differences between conditions at every stage of the series. In all cases their chance probability is less than one in a thousand. There is thus little evidence for "coding," i.e. for rendering the conditions equivalent in terms of the information which can be derived from them. All that the subject has to do with the true affirmative and false negative conditions is to select the colours in the statement which correspond to those in the relevant quadrants of the situation. All that he has to do with the false affirmative and true negative conditions is to select those colours which do not so correspond (or to select one colour which does not correspond and the other at random). But it does not seem as though subjects were able to interpret the conditions in this way, for had they done so, the response times to all conditions would have tended to converge.

Table III shows that on only one trial (i.e. the second), does the mean response time for the true affirmative condition exceed that of any other condition. It will also be seen that the true negative response time is longer than that of the false affirmative (and true affirmative) in 18 of the 24 trials. The false negative response time is also longer than that of any other condition on 18 of the 24 trials.

It should be noted that the form of statement (affirmative or negative) appears to exert a more determinate influence on response than its prescribed truth value (agreeing or conflicting with the situation).

TABLE IV

ANALYSIS OF VARIANCE, BETWEEN CONDITIONS

Second Presentation of Conditions:

Effects	Sum of Squares	d.f.	Variance	P
Conditions	4·6069	3	1·5356	< 0·001
Within Groups	1·9911	92	0·0216	
Total	6·5980	95		

Third Presentation of Conditions:

Effects	Sum of Squares	d.f.	Variance	P
Conditions	0·7370	3	0·2457	< 0·001
Within Groups	2·0127	92	0·0219	
Total	2·7497	95		

Fourth Presentation of Conditions:

Effects	Sum of Squares	d.f.	Variance	P
Conditions	0·4478	3	0·1493	< 0·001
Within Groups	1·4738	92	0·0160	
Total	1·9216	95		

Fifth Presentation of Conditions:

Effects	Sum of Squares	d.f.	Variance	P
Conditions	0·4160	3	0·1387	< 0·001
Within Groups	1·2620	92	0·0137	
Total	1·6782	95		

Sixth Presentation of Conditions:

Effects	Sum of Squares	d.f.	Variance	P
Conditions	0·3930	3	0·1310	< 0.001
Within Groups	1·1869	92	0·0129	
Total	1·5799	95		

2. *Transfer and learning*

Four two-way Analyses of Variance were computed to examine (*a*) the effects of practice on each condition, and (*b*) transfer effects from the immediately preceding condition on each of the four conditions.

The results showed that the effects of practice were significant for all conditions, but at different confidence levels. For the true affirmative condition this level is less than 0·025, for the false affirmative less than 0·05, for the true negative less than 0·001 and for the false negative less than 0·05.

There are no significant transfer effects and no significant interactions between transfer and the effects of practice. In other words, the immediate previous experience of any condition has neither a facilitative, nor an inhibitory effect on response to any other condition. The main parameters of response times are the particular condition which is operative and the general effects of practice on preceding trials.

3. *Errors*

The total number of errors made on each condition (out of a possible 144) is as follows, true affirmative (4), false affirmative (6), true negative (8) and false negative (29). Table V shows the frequency of errors at each successive presentation of conditions.

TABLE V

FREQUENCY OF ERRORS FOR EACH CONDITION AT EACH SUCCESSIVE PRESENTATION
(max. possible at each presentation = 24)

	PRESENTATIONS						
	First	*Second*	*Third*	*Fourth*	*Fifth*	*Sixth*	*Total*
TRUE AFFIRMATIVE	1	2	1	0	0	0	4
FALSE AFFIRMATIVE	2	0	1	0	2	1	6
TRUE NEGATIVE ..	5	1	1	1	0	0	8
FALSE NEGATIVE	13	6	5	1	3	1	29
TOTAL ..	21	9	8	2	5	2	

The striking difference between the number of errors in the false negative condition, compared with the others, reflects its great phenomenal difficulty which many subjects reported. More than half the subjects (13) made errors on it at its first presentation. For these errors there was a small but significant correlation (*tau* = + 0·35, P = 0·04) (Kendall, 1948) between response time and successive presentation, i.e. as the series progressed the time taken for incorrect responses to false negatives tended to decrease. And this suggests that these errors were not due to random behaviour but were made after effort at comprehension. (Errors were too few under the other conditions to establish valid correlations.)

There is a general decline in errors for three of the conditions with practice, the exception being the false affirmative on which accuracy was more variable. And yet no knowledge of results was given. The explanation must be that understanding of what the tasks involved increased gradually. This might entail a sort of rudimentary coding of the "meaning" of the conditions, not necessarily amounting to conscious awareness or verbalization, by virtue of the fact that there can be only two kinds of response, although there are four kinds of condition. But this coding is not strong enough to make the response automatic; it is only strong enough to increase its accuracy. For instance, most subjects reported (*see* following section) that they suddenly realized that the true affirmative was "the same as" the false negative about half-way through the series.

4. *Logic of response*

Only five out of the 24 subjects deliberately made the logically sufficient response to the negative information. And only two of these five (and they had some knowledge of formal logic) made this response in the most efficient way. The remaining 19 subjects all responded to negative information (as revealed by their responses, demonstrations and introspections) by segmenting the statement into two halves and treating them independently. In other words, they construed the negative information as if it possessed the form "neither . . . nor. . . ." The meaning of "not both" was lost on them. They endeavoured to leave both components unfulfilled, instead of one which would have been logically sufficient.

The most efficient strategy for dealing with negative information is, of course, to look at only the first-named quadrant, make the appropriate selection of alternatives, so that the colour word in the first component is not the same as the colour in the situation, and then select an alternative in the second component at random. Two subjects, as mentioned above, did just this; of the remaining three, one subject made both components different from the situation in the false affirmative condition and one different and one the same in the true negative. The second subject adopted the same procedure for false affirmatives and for true negatives by making the alternative in the first component always the same as that in the situation and the second always different. The third subject adopted the most bizarre procedure. He selected the alternative in the top position of the first component of the statement, then looked at the situation to see whether his selection was the same or different from it, and finally made a selection from the second component so that the result showed one alternative the same as that in the situation and one different. In other words, his second choice was identical to the situation, if his first one was not identical and *vice versa*. Finally, one of the 19 subjects, who always tried to make both components different from the situation, said that he realized that only one need be so, but preferred to have them both different.

A majority of subjects (13) reported that at different stages in the experiment they realized that the true affirmative and false negative conditions were equivalent. Six reported that they never realized the equivalence and had to "work it out" each time, and four reported that they realized the equivalence in the first presentation of the false negative.

5. *Self-assessment of performance*

The individual rank correlations (*tau*) between mean performance on the conditions and self-assessment of their performance ranged from + 1·0 to − 0·17, with a mean value of + 0·63. Thirteen of these correlations were + 0·67, five were + 1·0, two were + 0·33, two were 0, one was + 0·83 and one was − 0·17.

Discrepancy between performance and its assessment was nearly always due to failure to discriminate accurately between the two negative information conditions, rather than between the two positive ones. The mean performance on false affirmatives was superior to that of true negatives for 20 out of the 24 subjects. But half the subjects (12) assessed the true negatives as easier than the false affirmatives although on mean performance the false affirmatives were superior in 11 cases and the true negatives superior only once. The other half of the subjects assessed the false affirmatives as easier than true negatives, while their performance showed the false affirmatives superior in 9 cases and the true negatives superior in three. Thus, for negative information there is no significant relation between performance and its assessment. In the correlations the two kinds of negative information are transposed in 14 out of the 24 cases, i.e. in 14 cases, if one kind of negative information was superior in the performance, the other kind was assessed as easier. It is also interesting to note that those subjects (five), who actually commented that they found true negatives easier than false affirmatives, in fact gave better average performance on false affirmatives.

There is considerable evidence to suggest that responding to negative information in the logically sufficient way is detrimental to the assessment of performance. The mean correlation between performance and assessment for those subjects who did not respond in the logically sufficient way is + 0·71. The corresponding figure for those who did respond in the logically sufficient way is + 0·30 (the individual values being + 1·0, + 0·67, 0, 0, and − 0·17). Thus three of the subjects who responded in the logically sufficient way obtained the lowest correlations between performance and its assessment. One of the zero correlations was due to considerable under-estimation of the difficulty of the true negative. The other zero correlation was obtained by a subject who had adopted the most efficient strategy for dealing with negative information. And in this case the lack of correlation was due to subjective over-estimation of the difficulty of the false affirmative and subjective under-estimation of the difficulty of the false negative. The correlation of −0·17 was also obtained from a subject who adopted the most efficient strategy and this was due, significantly enough, to tying the two kinds of negative information in first place in the subjective assessment, whereas, in fact, their rank position on mean performance was two and four.

It is important to note than the true affirmative condition gave the best performance for both the subjects who adopted the most efficient strategy for negative information. And this in spite of the fact that two quadrants had to be looked at in this condition, as opposed to only one in the negative information conditions. One of these subjects assessed the true affirmative as easiest, but the other placed it third in order of preference.

DISCUSSION

There are at least three explanations which could account for the results of this experiment, the first two of which are rather closely related. The first explanation (not necessarily the most important one) assumes that there is an acquired tendency to respond more readily to positive than to negative information because of a long-standing investment in it. The second explanation is concerned with the inferential nature of negative information in relation to perceptual experience, and the third with the possible emotional effects which may be aroused by negative terms. These explanations will be discussed in turn.

There is far more involvement and interest in information which asserts or implies that something is so than in information which denies that something is so. And this is obviously because positive information is more valuable in adapting to the environment. It is, on the whole, more vital to find out what is the case (usually expressed affirmatively) than to find out what is not the case (usually expressed negatively). Thus in most kinds of discourse (with the probable exception of legal documents, regulations, etc.) affirmative statements are more frequent than negative ones. In gaining new knowledge or information, in fact, a denial is useful only if it can clarify positive information, as it does in a comparison. For instance, in expounding X it may be necessary to draw attention to the fact that it is unlike Y in such and such a respect but the aim of the exposition is to clarify the nature of X, so that a true affirmative statement could be made about it, a statement which would generally be more comprehensive and "meaningful" than a negative one. Negative statements, therefore, perform only the subordinate function in assertive discourse of excluding error in the acquisition of positive information. They are useful only in so far as they can be turned to positive account.

More important, however, than the frequency and interest in positive information, is the fact that there would appear to be a necessary under-valuation of negative information in early learning. Concepts are not learned in isolation from each other. For instance, the class of "cats" is presumably first introduced by exhibiting a positive instance. But if, subsequently, a child were to say "cat" on the appearance of a dog, the adult would not say "not cat," but "dog." The occurrence would be taken as an opportunity to teach a new concept. Thus the negative instance of one concept becomes immediately assimilated to the positive instance of another. It is desirable not to establish a persistent interest in the class of "not cats." Hence the utilization of negative information would seem to be a relatively sophisticated and mature activity because it implies a store of positive information which can be used as a standard. It is no good knowing what something is not unless that helps to eliminate possibilities about what it is.

The interest in positive information, its frequency of use, and the emphasis given to it in early conceptual learning, make it reasonable to assume that there is a learned tendency to respond to it more readily than to negative information. It may also have the effect of making true (and affirmative) statements seem intrinsically more valuable, and consequently more acceptable, than false (and negative) ones. As one subject remarked, "The natural inclination is to accept what is the case regardless of any instructions."

It seems plausible, therefore, to suggest that the nervous system does not behave like the Kalin-Burkhart Logical-Truth Calculator but, as far as speed goes at any rate, responds in a one-sided way in favour of positive information, just because that kind of information is of such practical importance. In other words, it is assumed that there is an habitual (and unnoticed) positive set to the environment which makes adjustment to negative information "inherently" slower.

Such an assumption would account for Hovland and Weiss' (1953) finding that it is more difficult to attain concepts from negative instances alone than from positive instances alone, even when the frequency and amount of information given by the instance is controlled. It would also throw light on Bruner's (1956) finding that subjects are not content to make inferences from negative instances, but redundantly prefer to make "direct tests" from positive ones.

In the present experiment the assumption of a "pre-existing" set for positive information would explain why the false affirmative condition takes longer to process than the true affirmative one. The explanation of the results of the other two conditions would follow by virtue of the fact that the order of presenting (i) the instructions (giving the truth value), and (ii) the statement is not varied. The instructions to make the statement "agree" or "conflict" are naturally read before the affirmative or negative statement. And this means that the instructions and the statement together induce a combination of positive and negative sets, but that the order of inducing them is constant for each condition. For the true affirmatives no "inversion" of set is required at all. The assumed, habitual set is first of all reinforced by the instructions, i.e. put into a state of greater readiness and subsequently maintained by the affirmative statement which follows. For false affirmatives, on the other hand, the instructions cancel the habitual set and substitute a negative one which is maintained by the affirmative statement. For true negatives, the assumed, habitual positive set is first of all reinforced by the instructions and then inverted to a negative set by the negative statement. Finally, for false negatives the habitual set is first of all converted to a negative one by the instructions and then changed back to positive again by the negative statement. The introspections of those subjects, who did not use the logically sufficient response for negative information, confirm this analysis. "There is something more awkward about having a negative in a statement than in an instruction. One is not so used to seeing it. One tends always to see a statement as 'there is,' rather than 'there is not.' " "After seeing 'conflict' I did not have to re-arrange my ideas at the statement. I did not have to reorientate myself."

"I prefer to have the negative first. I then feel I have an idea in my head and know what I have to do. 'Agree' and then 'not' pulls me up." "It is easier to get set for 'Agrees,' but then not so easy to stamp it out for 'not.' With 'conflict' at the top I did not have to do any mental juggling at all."

On the other hand, one subject, who did adopt the logically sufficient response for negative information, said, "When 'conflict' appeared at the top, I made the statement into 'there is not.' " The remarks of two other subjects, who also adopted the logically sufficient response, are particularly interesting. Both these subjects assessed true negatives as easier than false affirmatives but both, in fact, gave a superior performance with false affirmatives. Because of their subjective assessments, however, and the fact that they had appreciated the logic of "not . . . both," they were able to "rationalize" their real (or apparent) discomforture with the false affirmatives: "It is easier to hold instructions which specify agreement, rather than those which specify conflict." "With 'conflict' one feels more hesitation. You have to see what the situation is first and then turn it round. This does not apply to 'Agree' and then 'not' where you just tick off." (Nice illustrations of the fallability of introspection.)

Another subject, who reported a feeling which she described as "panic," confused the required correctness of solution with the "incorrectness" inherent in negative information. "I look at the instructions and it might say do something to make the statement wrong. But I wanted to get the task right. There are two levels of thinking: what I have to do to be right and what has to be done with the information."

To summarize, the number of inversions between sets and the order in which sets are induced account for the results, if an habitual positive set can be assumed. There is no inversion of set for the true affirmative and there are two for the false negative. The false affirmative and true negative both require a single inversion. But the difference between them lies in the order of the sets. In the false affirmative the initial inversion to negative is simply maintained by the affirmative statement, but in the true negative the initial reinforcement of positive set makes the subsequent inversion to negative more difficult.

The second possible explanation of the results (which is related to the first one) is concerned with the inferential nature of negative information. A true affirmative statement corresponds to perceptual experience in a way in which a false affirmative or true negative one does not. It resembles the form of a protocol: "a direct statement of the given." A false affirmative only implies the non-occurrence of a particular experience and a true negative expresses this fact directly. But in both cases these statements do not define what *is* experienced. The truth of an affirmative, on the other hand, depends on a one-to-one correspondence between situation, percept and statement. But a negative statement is true, and an affirmative one false when the situation to which they refer exhibits any value, other than the one named in the statement. As Ayer (1952) puts it: "Logically a negative statement, assuming it to be relatively unspecific, can be verified only through the truth of some more specific statement which entails it; a statement which will, itself, by contrast, be counted as affirmative."

In the present experiment if, for instance, there is red in 3 and the condition is one of negative information, the subject has to select the colour word which is offered with the colour word "red." But this alternative colour word may be "black," "green" or "yellow" and it has, presumably, to be inferred that any of these colours is equivalent to "not red." The subject has to be prepared to accept the one of three possible alternatives which is offered in order to fulfil the negative information. Nevertheless, it is hard to see why this causes difficulty because it does not matter which colour word is mentioned with "red." All that the subject has to do is to encircle the word other than the one corresponding to the colour in the appropriate quadrant, without necessarily noticing what this alternative is. Knowing that the word is, say, "black" rather than "yellow" is useless knowledge. The point is that it is "not red." That it did evidently cause trouble, to one subject at any rate, is illustrated by the following comment, "one cannot use a binary coding because 'black' does not always equal 'not yellow.' " (This subject also made the interesting observation that at one stage she suffered from a complete block and could not decide which colour was green and which was yellow.)

There are really two factors here. One is the number of affirmative statements which are allowed to entail a single negative statement. The other is the fact that, even if the situation can exhibit only two states, *x* and *y*, i.e. if *x* is equivalent to not-*y* and *y* equivalent to not-*x* (e.g. the two sides of a penny), an inference is still needed to get from an affirmative statement to the negative one. In other words, if a subject is told to respond in a particular way to "not-tails" and "heads" turns up, he must get from the implicit affirmative statement "there is heads" (which corresponds to his experience) to the negative statement "there is not tails" before he responds. Whether this inference could lead to a reaction, as rapid as that to an affirmative statement, even with continuous practice, is not known. It seems highly probable, but it is not certain.

It is possible to design an experiment in which an affirmative statement is allowed to be fulfilled by many different events sharing a common property and a negative statement allowed to be fulfilled by only a single event, i.e. an experiment in which the normal range of conditions for fulfilling affirmative and negative statements is inverted. There might be two classes, one, say, of odd numbers and one of even numbers. The statements would be of only two kinds: "There is an odd number" and "There is not an odd number." But the situations (i.e. the display) would consist of either different odd numbers, or of one particular "not odd number," e.g. the number "four." At any stage the conditions could be inverted back to those which normally obtain, i.e. one particular instance for affirmative statements and different ones for negative statements. Thus the lack of specificity which a negative statement usually possesses might be controlled.

The fact that a negative term does not belong to a direct description of experience may be part of the reason why it is impossible to ask a question in English about a negative state of affairs without circumlocution. There is no contradiction in saying either "yes" or "no" to both the following questions: "Is it red?" and "Is it not red?" because the "not" does not refer to an assumed lack of redness but is rhetorical. "Is it not red?" means "Is it not true that it is red?" and not "Is it true that it is not red?" This fact has the odd consequence that the questions, "Is it not true?" and "Is it false?" although syntactically synonymous, pragmatically have the opposite meanings.

The third explanation of the results is rather different from the first two. It is concerned with the emotional response to negatives. Words which imply positive actions or states of affairs have, in general, a more pleasant or creditable connotation than their opposites. "Yes" is permissive; "no" prohibitive. To agree is more pleasant than to disagree; to advance more creditable than to retreat, etc. It seems possible that the unpleasant hedonic value of the negative is sufficient to inhibit momentarily the response. Some of the subjects' comments are relevant: "The instruction 'Conflict' looked more terrible than 'Agree.' " " 'Not' gave me a sort of tremor half-way through." "I don't like 'not'—it's a horrid word." "The capital letters of 'not' always frighten one."

It is, of course, possible to express negative information without explicit use of the negative. Thus the language used in the present experiment could be transposed so that "not" appears in the instructions rather than in the statement, e.g. "Adjust the statement so that it is not true," followed by a statement in the form, for example "red is absent (or present) in 3." It is also possible to express a specific statement, i.e. one which is true for only one value in the situation and false for *n* values, in a negative form; and to express an unspecific statement, i.e. one which is true for *n* values in the situation and false for only one, in affirmative form. The result is rather unnatural but not ungrammatical. For example, the statement, "There is not any colour in 3 except red," is in a negative form, but it is true if and only if red is in 3 and otherwise false. Similarly, the statement "There is some colour in 3 other than red," is in an affirmative form, but it is false if and only if red is in 3 and otherwise true (assuming, of course, that some colour always appears in 3). An experiment which uses language in this way is similar to the one cited previously which defined a class of odd and not-odd numbers. But in that experiment the number of situations which fulfilled affirmative and negative statements was arbitrary. Here, in contrast, only one situation can make the negatively expressed statement true.

It can also be argued, not that the words used in the present experiment were emotionally disturbing but that they were rather difficult to grasp. That this was indeed the case is shown by the fact that 19 subjects missed their point. The construction "not both" is certainly rather unfamiliar. It is probably more difficult to grasp the difference between "There is both" and "There is not both," than it is to grasp the difference between "There is" and "There is not." Several subjects complained that the word "both" was troublesome, in fact one covered this word with her finger at every trial. It is certainly plausible to attribute some of the results of the first four trials to difficulties of understanding words. But it seems rather less likely that subjects have continually to strive to understand the meaning of the words used in instructions and statements, as opposed to grasping their joint implication. Those subjects, who did not adopt the logically sufficient procedure for negative information, imposed their own meaning on the conditions and seemed to adjust to the experiment quite happily by segmenting the statement and assuming that the negative qualified both its components, i.e. by construing the statement as "neither . . . nor." Those subjects, on the other hand, who did adopt the logically sufficient response for negative information, and hence were aware of the differential probability of correct answers for the two kinds of information, were emphatic that they experienced no conflict or competition in selecting their answers to negative information.

But it cannot, of course, be deduced from the present results that it is more difficult to identify true negative statements than false affirmative ones because the order in which instructions and statements are read is constant. The instructions, which specify truth value, are always read first. To establish such a conclusion it would be necessary to have a temporal interval between instruction and statement, and to vary the order of presentation.

This experiment has examined the case in which affirmative and negative conjunctive statements have to be identified in accordance with instructions to make them true or false. It would be interesting to determine whether the same results are found in the verification of affirmative and negative statements. Instead of adjusting a statement so that it is true or false, the task would be to determine whether the statements are true or false, i.e. to select the response "true" or the response "false." It is possible, if necessary, to complicate this task by introducing a further step in the processing of the information. This could be done by (*a*) asking either "Is the statement true?" or "Is the statement false?", (b) presenting affirmative or negative statements, and (*c*) varying the situation so that the correct answer is either "yes" or "no."

It is evidently possible to exert some control over two of the variables which are usually associated with negative information:

(*a*) the linguistic factor, i.e. the actual words used to denote negation, and

(*b*) the degree of specificity of negative statements, i.e. the range of situations which are allowed to fulfil them.

By manipulating these variables systematically it should be possible to assess their contribution to the apparent difficulty of dealing with negative information. It might then be shown under what conditions the nervous system fails to behave like a computer, whether it could be trained to respond mechanically, or whether the inevitable inferential nature of negative information and the persistent interest in positive information puts it for ever at a disadvantage.

I am indebted to Mr. J. W. Whitfield, who first interested me in this area of research, for his encouragement and constructive criticism, to Mr. J. Irwin for assistance with the design of the experiment and to the students who so readily agreed to act as subjects.

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SHORTER ARTICLES AND NOTES

INTELLIGENCE LEVEL AS A VARIABLE IN THE PERCEPTION OF SHAPE

BY

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From the University of Wisconsin

The relation between shape constancy and intelligence was investigated by determining the functions relating matched shape to the angle of inclination of a circular test-object for groups at four levels of intelligence. Subjects were obtained from groups of institutionalized mental defectives, slow learners from the public schools, unselected undergraduates, and undergraduates who had been awarded scholarships. With increasing intelligence level the matches tend to approach the prediction based on the law of the retinal image thus demonstrating an inverse correlation between intelligence and the extent to which constancy is demonstrated. Since it is known the instructions given to the subject are a variable in shape constancy, it is suggested that the present data are the result of different attitudes adopted by the subjects at the various intelligence levels. The more intelligent subjects are assumed to adopt an analytic attitude in the experimental situation, while the less intelligent tend to maintain their everyday, non-analytic modes of perceiving.

INTRODUCTION

Experimental studies of perceptual constancy typically demonstrate high intersubject variability (Klimpfinger, 1933; Sheehan, 1938; Thouless, 1932). In the perception of shape, this variability increases under conditions which favour the appearance of the constancy effect. With increase in exposure duration or luminance, shape matches exhibit an increasing tendency toward shape constancy as well as greater intersubject variability (Leibowitz, Mitchell & Angrist, 1954; Leibowitz & Bourne, 1956). The purpose of the present experiment is to determine the role of general intelligence as a factor in shape matching under experimental conditions which produce high variability among subjects. The tendency toward shape constancy is measured by asking subjects to match a disc, mounted on a turntable at various angles to the line of vision, with one of a graded series of ellipses (Leibowitz & Bourne, 1956). Intelligence level is introduced by utilizing four groups of subjects consisting of (1) mental defectives of the familial type, (2) retarded students from the public (i.e. State) schools, (3) university undergraduates, and (4) undergraduates who have been awarded fellowships by the Ford Foundation. The function relating matched shape to angle of inclination of the test-object is determined for all subjects under identical stimulus conditions.

APPARATUS AND PROCEDURE

The apparatus, which has previously been described in detail, consists of a modified Dodge transparent mirror tachistoscope (Leibowitz & Bourne, 1956). In one field, a white disc 3·0 cm. in diameter and 2·0 mm. thick is mounted on an adjustable turntable. In the other field one of a graded series of ellipses, drawn with black ink on white vellum paper, appears. This series is arranged in order from a circle to an ellipse for which the axis ratio (ratio of minor to major axis) is 0·054. The centres of the two fields are 9·6° apart. The fields appear alternately, the disc for 1·0 sec. and the ellipse for 1·4 sec. The subject views binocularly and is instructed to choose the ellipse which "looks the most like the shape of the disc." Except for the feebleminded group, the subject manipulated knobs to bring the appropriate ellipse into view. For the feebleminded group, the experimenter adjusted the knobs and the subject signalled whether the visible ellipse was the same or different from the disc. When the response "same" was given, the experimenter continued to change the ellipse until the response "different" was again elicited. The subjects were instructed to take as much time, i.e. as many exposures of the disc and ellipses, as was required to make a satisfactory match. Each subject made three matches at each of seven angles of inclination of the turntable, the order being determined by a 7 × 7 latin square design. Between matches, the experimenter offset the ellipse series randomly. The illuminance incident on the stimulus and drawings was 10 foot-candles. A session lasted from 15 to 30 min.

SUBJECTS

The first two groups tested, the Ford fellows and the mental defectives, were chosen to represent as great a difference in intelligence as possible. The Ford fellows consisted of twenty-one males and twenty-one females who had been awarded "early admission" fellowships on the basis of superior academic potentiality. These students entered the University of Wisconsin before completing their last year, or in some cases their last two years, of high school. As a group, their performance in the university has been unusually good.

At the other extreme, an attempt was made to choose subjects from the Southern Wisconsin Colony and Training School whose intelligence was as low as possible but who could be tested on the same apparatus as used with the other subjects. Preliminary experiments indicated that subjects with mental ages below seven years could not understand and/or follow the instructions. The eleven females and ten males chosen were of the familial type and had no history of brain injury, abnormal EEG, eye defects, or other pathology. All co-operated well in the experimental situation.

When it became apparent that there was a difference between the matching functions obtained from the extremely high and low intelligence groups, two additional groups at intermediate levels were introduced. Twenty-one "slow learners," eleven females and ten males, were obtained from the remedial classes of the local public schools. These subjects have an IQ range from 61 to 88 with a mean of 74 as compared with the institutionalized subjects range of 51 to 80 with a mean in the low 60's. A fourth group whose general intelligence level may be assumed to be between the "slow learners" and the Ford fellows was obtained by requesting volunteers from the undergraduate psychology classes at Wisconsin. Ninety-eight of these subjects, equally distributed as to sex, were tested. For all groups, the subjects were unaware of the purpose of the experiment.

TABLE I

MEAN MATCHED RESPONSE AXIS RATIO AS A FUNCTION OF ANGLE OF INCLINATION OF A CIRCULAR TEST-OBJECT FOR FOUR GROUPS AT VARIOUS LEVELS OF INTELLIGENCE

	Angle of inclination (degrees)						
	10·0	18·5	24·0	34·0	45·5	59·0	90·0
Stimulus axis ratio	0·174	0·317	0·407	0·559	0·713	0·857	1·000
Mental defectives							
N = 21	0·778	0·824	0·865	0·880	0·903	0·904	0·948
Standard deviation ..	0·294	0·234	0·177	0·142	0·109	0·095	0·046
"Slow learners"							
N = 21	0·422	0·645	0·699	0·803	0·796	0·865	0·960
Standard deviation ..	0·288	0·247	0·216	0·135	0·110	0·081	0·044
Elementary psychology Students							
N = 98	0·350	0·561	0·677	0·790	0·868	0·919	0·999
Standard deviation ..	0·206	0·203	0·183	0·126	0·095	0·079	0·027
Ford Fellows							
N = 42	0·316	0·526	0·620	0·753	0·855	0·919	0·985
Standard deviation ..	0·093	0·123	0·131	0·142	0·099	0·061	0·025

Results

The mean matched response axis ratios and their standard deviations are presented for all groups as a function of the angle of inclination of the test-object in Table I and plotted in Figure 1. The response axis ratio is the ratio of the minor to the major axis of the matched ellipses. If there were no tendency toward perceptual constancy, the matched responses would be equal to the stimulus axis ratio, or the calculated ratio of the same axes of the test-object projected on a vertical plane. Such data would fall on the line in Figure 1 labelled "law of the retinal image." At the other theoretical extreme, the law of shape constancy, which predicts that the matched response should always be circular (ratio of 1·000), is represented by the horizontal line in the figure. As is typical in studies of perceptual constancy, the matches represent a "compromise" between the two theoretical extremes. For all groups, the chosen ellipses are rounder than would be expected from geometrical optics. However, the extent to which the data exhibit the tendency toward constancy depends upon the intelligence level. The highest intelligence group, the Ford fellows, produce matches which lie closest to the prediction in terms of the shape of the retinal image while the lowest intelligence group, the mental defectives, produce matches which lie closest to the law of shape constancy. Data for the groups of intermediate intelligence lie near to those for the Ford fellows with the more intelligent group, the university undergraduates, being closer to the law of the retinal image. Thus, the four groups show an inverse correlation between intelligence and the extent to which their data approximate the law of size constancy.*

FIGURE 1

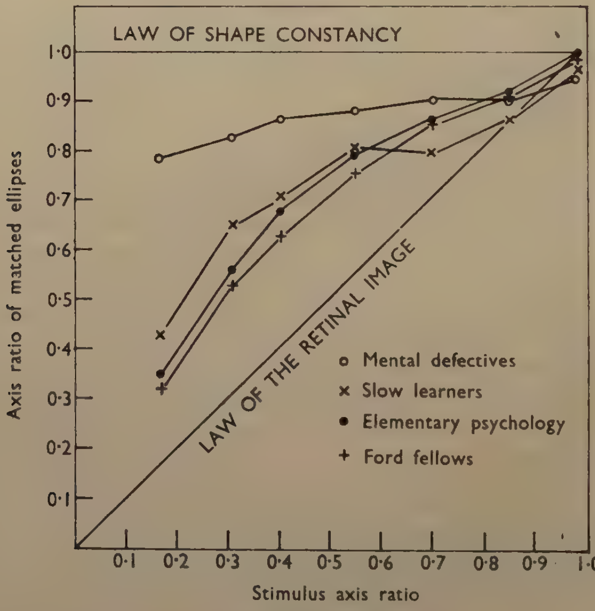

FIGURE 1

Mean axis ratios of matched ellipses as a function of the stimulus axis ratios of a disc test object presented at various angles of inclination for groups at four levels of intelligence.

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* Complete data for the individual subjects in all groups may be obtained on application to the authors or to the Editor of this *Journal*.

The data were analysed by means of a modified Alexander trend test (Grant, 1956). The difference between the linear component of the trends for the four groups was significant beyond the 0·01 level of confidence. In order to determine what differences among individual groups was responsible for this trend, a Duncan range test was used (Duncan, 1955). By means of this test, it was possible to evaluate the significance of the differences between the slopes of all possible pairs of groups. These differences were found to be significant for all comparisons except that between Ford scholars and elementary psychology students. With this exception, the statistical tests support the impression obtained from the graph that the extent to which shape constancy is demonstrated is inversely related to intelligence.

Discussion

The results of the present experiment indicate that with increasing intelligence level the shape matching functions exhibit a decreasing tendency toward shape constancy. For a given angle of inclination of the circular test-object, less intelligent subjects tend to choose a "rounder" ellipse of greater axis ratio. Similar results obtained over a less extensive range of conditions have previously been reported by Thouless (1932).

Since the stimulus conditions were identical for all subjects, any discussion of the present results must be in terms of the conditions of the organism at the time the discrimination was made. A number of experimenters have reported that the instructions to the subject, or perhaps more precisely the set or *Einstellung* resulting from the instructions, can influence the values obtained in shape matching studies (Angrist, 1954; Klimpfinger, 1933). Such findings provide a possible basis for interpretation of the present data. The instructions in the present experiment require the subjects to choose an ellipse such that the test-object and the chosen drawing appear the same. The subject is being asked to adopt an analytical attitude toward the test-object in order to produce a match between a two-dimensional drawing and the tilted disc. If it be assumed that the more intelligent the subject the more able he is to follow the instructions and adopt an analytic attitude, the present findings may be understandable in terms of the known effects of set or attitude as a variable in shape perception. Stated differently, with increasing intelligence the subjects are more able to follow the analytic instructions and to produce a retinal image or geometrical match. Contrariwise, the less intelligent subjects are less able to shift from everyday, non-analytic modes of observation in order to comply with the requirements of the experiment.

Such an analysis implies that the results of the present experiment are an outcome of the reaction of subjects at various intelligence levels to the experimental situation. A disadvantage of this interpretation is that it is not now possible to specify the correlated conditions of the organism. However, the empirical demonstration that intelligence is an important variable in the perception of shape is at the present stage a first step toward providing the basic data with which such specification will be concerned.

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A BLUE FILTER TECHNIQUE FOR DETECTING EYE MOVEMENTS DURING THE AUTOKINETIC EFFECT

BY

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It is well known that if a stationary small dim light source placed in a dark room is fixated it will, after a time, appear to move. This apparent movement ("autokinetic sensation") is compelling, it cannot be arrested by the will (as Helmholtz might have put it) and it is seen by all observers in the appropriate conditions. The movement may be erratic or fairly steady, fast or slow. It may cover angles of well over 30°. When large, the effect gives a feeling of paradox—the light moves, yet one does not seem to follow it with the eyes and hence it cannot have moved.

In a recent textbook, Walsh (1957) writes: "The *autokinetic sensations* . . . are probably produced by slow movements of the eye . . . these sensations represent an imperfection of the visual apparatus, for if the central apparatus had available and successfully dealt with accurate data concerning the position of the eyes, arrangements could be made to 'cancel out' the effects of the shift of the image across the retina."

This seems a surprising statement in view of the now classical paper of Guilford and Dallenbach (1928) who undertook elaborate experiments with a photographic technique to detect and measure eye movements during the autokinetic effect. Guilford and Dallenbach could find neither large slow movements, nor small "saw-tooth" movements which might, by some kind of summation, have produced the autokinetic movement.

The detection of eye movements in the dark is a nasty problem. A convenient method would have many uses, besides serving as a simple check on Guilford and Dallenbach's results. The following method is simple, the apparatus costs very little, and it may be used for at least one nice class demonstration and for various research purposes.

METHOD

It is fairly well known that the *fovea* of the human retina is blind to weak stimulation by far blue light (Willmer, 1946). The area involved is at the region of central fixation, where acuity is greatest for bright white light. This central blue-blind spot may be demonstrated by placing a blue filter (the Ilford Narrow Cut Spectral Blue is suitable) in front of a small dim light source. If the intensity is suitably adjusted, the blue light will be seen clearly in peripheral vision, as a bluish-white light, but will be invisible to direct fixation. By adjusting the subtended angle of the blue source a small eye movement may be detected, for immediately the eye shifts from direct fixation this bluish-white is seen. After a few minutes dark adaptation the effect is striking. It is even possible, after some practice, to see the edge of the blind area moving about on the blue field with the very small saccadic movements of the eye.

In order to use this method to detect eye movements during the autokinetic effect, a small red filter may be placed in the centre of the blue field. The Ilford Narrow Cut Spectral filters are available as gelatin sheets, for photographic purposes; it is a simple matter to punch a hole in the blue filter and cut out a disk of the same size in the red, using the same punch. This red disk may be mounted in the hole in the blue filter by binding them in 2 in. × 2 in. cover glasses. This may then be mounted in a light box fitted with a torch bulb whose brightness can be controlled with a variable resistance. It will then be found that when the red light in the centre of the blue field is fixated, in a dark room, the usual autokinetic effect will be observed. If the eye is shifted from central fixation, the red light will be immediately surrounded by a bluish-white halo.

The question is: when the red light moves during the autokinetic effect does the halo appear? If it does, the eye must have moved; if it does not, then the eye cannot have moved sufficiently to shift the image of the blue from the blue-blind region of the retina. The sensitivity to eye movements may be adjusted by placing a photographic iris diaphragm over the filters and adjusting its aperture, and thus the angle subtended by the blue field, so that it fills more or less of the blue-blind region of the retina.

RESULTS

The autokinetic effect has so far been examined in fifty subjects with this device. None have reported the halo during apparent movements of the red light, though all reported seeing the halo quite distinctly when deliberately fixating a real or imaginary point just displaced from the red light. Thus large slow movements are evidently *not* the cause of the autokinetic effect.

By setting the aperture of the blue field so that its image covers the whole of the blue-blind region (so that sensitivity to eye movements is maximal) it should in principle be possible to examine one's saccadic eye movements while noting the drift of the red spot. When the eye is dark adapted, and the intensity and the angle subtended by the blue field adjusted optimally, the periphery of the blue field is seen to undulate in a curious manner, evidently indicating very small eye movements. These appear to be random rather than directed in any manner related to the direction of apparent movement of the red spot. But the observation is difficult to make.

These results confirm those of Guilford and Dallenbach using the photograph method. It seems quite clear that eye movements are not a necessary feature of the autokinetic effect, as may be very simply demonstrated with this blue filter technique.

I wish to thank my wife, Mr. Alan Watson, Miss Jean Wallace and Professor O. L. Zangwill for valuable discussion, advice and help. Funds from an M.R.C. apparatus grant were used for making the eye movement detector.

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APPARATUS

AN AIR DILUTION OLFACTOMETER SUITABLE FOR GROUP THRESHOLD MEASUREMENTS

BY

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An apparatus is described by means of which a series of smelling points may be kept independently supplied with constant controlled concentrations of odorous material.

It has been found by one of us (Cheesman & Mayne, 1953; Cheesman & Townsend, 1956), that threshold measurements made on groups of subjects may be made sensitive enough to enable small changes to be estimated with confidence. For the purpose of the experiments described in the publications cited, it was convenient to use a very simple technique in which the concentration of odorous vapour was controlled by means of an aqueous solution of the test substance. This method has the great advantage of requiring very simple equipment and a minimum of manipulation, but it is only reliable with materials which are completely miscible with water, and the choice of experimental materials is thus considerably restricted. In planning further work in this field, the present authors set about examining ways in which this restriction could be removed. A possible approach would be to replace water by some other odourless liquid diluent, but such a substance is not at all easy to find; even the few materials which conform to the requirements are open to the objection that they might have a measurable effect on the threshold of the odour of the solute even though they were undetectable by themselves. Although this argument may have little real force, it would need considerable work to place the matter beyond question, and it was concluded that it is preferable to dilute the odorous substance directly with clean air, and to avoid introducing any other material whatever.

Quite a number of air dilution instruments have been described in the literature (Woerdemann, 1934; Fair & Wells, 1934; Elsberg & Levy, 1935; Rennes, 1940; Jerome, 1942; Le Magnen, 1947; Wenzel, 1948; the last cited contains an excellent bibliography), but none appears suitable for determining group thresholds. Most workers have arranged their apparatus to deliver the various concentrations of odorant to the observer at a fixed point (a nosepiece or similar device). Such an arrangement has the merit of saving space and equipment, but with a group of observers this aspect is less important since replication of testing points is desirable in any case because it takes too long to test subjects one at a time. Once the replication of testing points is accepted, it becomes a distinct advantage to maintain steady concentrations at each of these points and to cause the subjects to circulate among them rather than to vary the concentrations reaching stationary subjects. By this means it is possible to eliminate uncertainty arising from the time taken for a changed concentration to become effective, a question closely bound up with the volume of the apparatus in relation to the rate of flow of air in it, and one which would impose very severe restrictions on the design of the apparatus. Thus the prototype apparatus described here was provided with twelve testing points and each observer sniffed at each of these points in turn during the complete test. Since the concentration of odorant remains constant at all points in the apparatus, the only instrumental time-effect was that of recovery of concentration in the smelling-point after it had been sniffed by a subject. This time of recovery could be made quite small by ensuring a reasonably high rate of replacement and a sufficient volume to act as a buffer. Likewise, effects due to the formation of adsorbed films on the glass surfaces reach a steady condition and exert no influence on the steady concentration of odorant. As individual subjects may have to be used in several tests in the course of time, it is desirable to have provision for changing the distribution of concentrations among the various smelling points, in order to eliminate any possibility of the subjects associating particular conclusions with particular situations.

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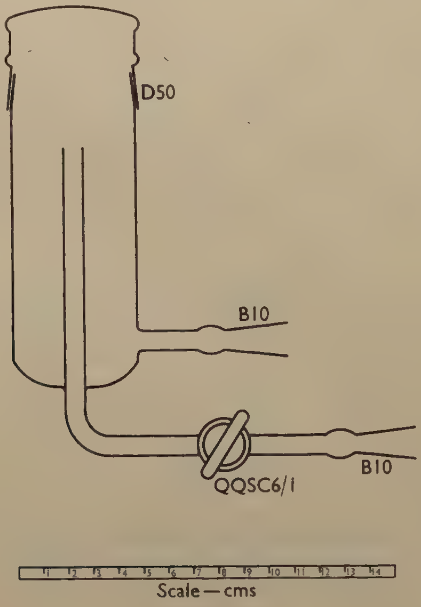


FIG. 1

Another aspect of design concerns the manner of driving the air through the apparatus; in many of the earlier olfactometers described in the literature this motive power has been provided by the observer himself. Usually a nosepiece fitted closely into the nostrils has been used and the sniffing of the observer drew air through the entire apparatus. In a group apparatus this introduces many difficulties and it is preferable to force the air through the apparatus from an external source, and to allow the subject to sniff in a natural manner. Apart from practical difficulties, some of our earlier experiments appear to indicate that the use of a nosepiece disturbs the observer's judgement; it is probable that each person unconsciously adopts a technique for sniffing and detecting odours with which he makes the best use of the olfactory faculty. The use of a nosepiece undoubtedly disturbs the mode of flow of the air through the passages of the nose and most observers show an appreciable reduction in sensitivity when first introduced to the use of a nosepiece. With continued practice they recover and possibly surpass their original sensitiveness; but as we have been concerned during our experiments with the average performance of a group of subjects whose history is not easy to follow in detail, we found it preferable to avoid any technique which introduces a marked training effect. Our smelling points were designed to simulate rather closely the bottles used in our earlier experiments, into which the observers were instructed to thrust their noses and sniff just as though they were trying to detect a smell in a bottle. Figure 1 illustrates the form of smelling point used and found satisfactory.

The present apparatus may be described in the following units: (1) the source of air; (2) an air-cleaning device to ensure that the air used was free of odour; (3) a saturator to provide a fixed upper concentration of odorant, and (4) a series of diluting devices supplying the appropriate concentration to each smelling point.

Air supply

As a source of air, a cylinder of compressed air was employed. A mechanical pump might be preferable, but freshly pumped air is apt to be badly contaminated with oil spray, and cylinders provide a convenient way of allowing the compressed air to stand for some time before use to allow fine droplets of oil to settle. Low-pressure gas-holders and fan blowers seem possible alternatives.

Air cleaning

A number of devices were tested for cleaning the air entering the apparatus. Carbon and silica absorbers were rejected after trial as rather slow and somewhat uncertain in action; they were apt to become saturated without warning. Considerably more success was found with a suitably designed condensing system cooled to a low temperature; this appears effectively to remove foreign odour from the air stream, provided that the rate of flow was low enough to enable all the air to come into contact with the chilled surface. Early designs consisted of copper tubing in which the area of cooled surface was increased by packing with metal turnings; and while these devices achieved a moderate success, it was found impossible to clean them satisfactorily. Glass apparatus was greatly superior in this respect as it could be cleaned chemically. Owing to the poorer conducting properties of glass it was necessary to increase the effective surface by using a number of fine tubes connected in parallel; a considerable length of tubing was incorporated in each element by coiling it up into a helix. In its final form the apparatus contained three coils, each consisting of about 2 m. of glass tubing with an internal diameter of about 4 mm., immersed in refrigerant. This apparatus cleaned compressed air from commercial cylinders quite effectively at 12 litres per min. when carbon dioxide-alcohol mixture was used, and could probably be used for still higher rates of flow if cooled with liquid air. Such a device is easy to cleanse by chromic acid and distilled water, followed by heating while passing a stream of dry air.

Saturator

Saturation of the air to the vapour pressure of the odorous substance at a controlled temperature provides a convenient way of producing a definite and reproducible concentration, and in order to reduce the amount of further dilution which has to be effected in order to bring the concentration down to the neighbourhood of the threshold value, it may be necessary to bring about this saturation of the air stream at a quite low temperature. The design of the saturator was basically that described by Lord Berkeley and E. G. J. Hartley (1908) for their experiments on the vapour pressures of aqueous solutions. A glass apparatus of this type but of slightly larger dimensions than that described in the original work was found to be effective (each chamber was about 25 cm. long and 2 cm. diameter). As reported by the original authors, agitation was found to be superfluous. It was tested by aspirating air at various speeds and measuring the uptake of vapour. It was found that the uptake of vapour fell very slightly with increase in rate of air flow, but up to the highest rates used in the olfactometer (about 1·5 litres per min.) the drop was only a few per cent. and could be neglected for our purposes.

At still higher rates of flow the fall became more marked, but it is believed that this effect may have been illusory, as a result of inefficient trapping of the vapour from the air stream in measuring the concentration at these high rates of flow.

This saturator was supported in a thermostat, and the incoming air was passed through about 70 cm. of tubing immersed in the bath before entering the saturating vessel proper. As the bath was intended for use at temperatures down to − 60° C., it was well lagged and the cooling was provided by circulating the bath liquid (alcohol) through a coil of copper tubing immersed in carbon dioxide-alcohol mixture contained in a large Dewar flask. The circulating pump was controlled by a thermo-regulator of conventional pattern, arranged so that the pump was put into operation as soon as the temperature rose above a predetermined value. Additional stirring was provided in the main bath and it was found that adequate lagging was essential in all cooled parts of the system. But without other precautions it was quite easy to maintain constant temperatures down to − 60° C. within about 1 degree, which was quite adequate for the purpose in view.

Dilution and mixing arrangements

The air passing from the saturator travelled through a considerable length of glass tubing to the next part of the apparatus. This lengthy connection served two purposes. In the first place it enabled the saturated air to regain room temperature; in addition it enabled the thermostat to be placed at a distance sufficiently far from the subjects that possible disturbance by the sound of the mechanism and the odour from the alcohol bath could be eliminated. In the particular arrangement with which these experiments were done, the saturator and air cleaner were placed in a small building on the roof of the testing laboratory.

Dilution

Upon entering the testing laboratory the tubes bringing the clean air and the odour-laden air were led along the centre of the testing bench and carried a number of branches. Each branch terminated in the concave member of a hemispherical ground glass joint.

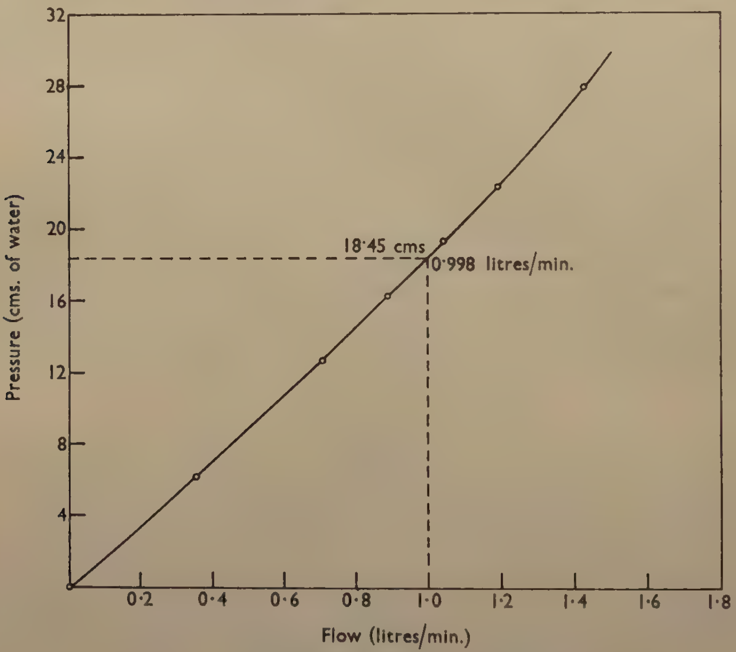


Fig. 2. Calibration curve for capillary A20.

The counterparts of these ground joints were attached to capillary tubes which had been calibrated for the rate of passage of air under a standard pressure difference. By this means each point on the air or odour distributing supply tube could be connected to a selected glass capillary tube which would take off a predetermined flow of pure air or odour-laden air as required. The distributing tubes must be of adequate diameter to ensure that all the diluting capillaries are fed at identical pressures. (In the original appratus tubes of about 1·5 cm. diameter were used, and these were not adequate. For a total flow of air of about 12 litre/min., tubes of about 2·5 cm. diameter or even more are desirable.)

The outlet ends of the capillaries were connected through similar ground joints to the smelling points arranged along the sides of the working bench. By locating the hollow hemispherical sockets with care, it was easy to ensure that the various calibrated capillary tubes were interchangeable at will, thus enabling any desired proportions of pure air and odour-laden air to be fed to each smelling point. Each capillary tube was calibrated at several pressures by measuring the volume of air passing per minute from the low-pressure gas-holder fitted with a constant pressure head. A representative calibration curve is shown in Figure 2 and Table I shows the values obtained for the whole set of tubes used in the apparatus at the standardized pressure difference.

TABLE I

Capillary Tubes

Designation: (A = pure air; O = odour-laden air; the members of a pair normally worked together are bracketed)	*Actual Dilution Ratio (intended ideal ratios in brackets)*	*Rate of air flow, litres/min. at* 18·45 *cm. water p.d. between ends*
A 1 A 2 A 3	used for supplying "blank" smelling points	0·997 1·005 1·01
A 4 O 5	1:7·55 (7·5)	0·878 0·1178
A 6 O 7	1:14·85 (15)	0·938 0·0632
A 8 O 9	1:29·88 (30)	0·9905 0·0332
A 10 O 11	1:42·2 42·42)	0·980 0·02325
A 12 O 13	1:58·0 (60)	0·951 0·01638
A 14 O 15	1:84·8 (84·84)	1·009 0·01190
A 16 O 17	1:123 (120)	1·015 0·00826
A 18 O 19	1:239 (240)	1·047 0·00438
A 20 O 21	1:492 (480)	0·998 0·00203

No. O 21 consisted of 10·2 cm. of 0·2 mm. (precision bore) pyrex tubing; wider capillary was used for the others and their lengths varied between 19 and 121 cm. Very fine tubes were avoided as far as possible owing to the serious effect of any partial blockage.

If the flow-controlling capillaries could be operated under conditions where the absolute rate of flow was strictly proportional to the pressure drop, the same dilutions would always be produced regardless of the absolute pressures existing in the distributing tubes of the apparatus, provided only that the pressures of pure air and odour-laden air were both the same. The rate of air flow was not exactly proportional to the pressure drop under conditions giving adequate rates of flow through capillary tubes with resistances which were large compared with the remainder of the apparatus. As will be seen from the curve of a representative tube (Fig. 2), the deviation is not great over the range of conditions used, but in order to eliminate error due to this effect, the apparatus was operated with a constant pressure in the distributing lines. This was achieved by allowing a slight excess of air to escape through a constant-head device at the remote end of each supply line.

Smelling points

The design of the smelling points gave a certain amount of difficulty, but after trying various arrangements it was found that a C55 tapered ground joint was large enough to be used in just the same way as the bottles in the earlier experiment. Furthermore, by using an outside-fitting cap in place of a conventional stopper, the disturbance of concentration due to turbulence when the vessel was opened was greatly reduced. The final form adopted for the smelling points (Fig. 1), therefore, consisted of a body of 55 mm. pyrex tubing about 10 to 12 cm. long with an inlet for the appropriately diluted odour near to the lower, closed end. A tube of about 8–10 mm. diameter passed concentrically through the closed end and extended to within 2·5 cm. of the open mouth, so that when the cap was in position the incoming air travelled the whole length of the chamber before escaping down the centre outlet tube. The caps were kept as flat as possible so that it was not necessary for the outlet tube to protrude beyond the open end of the C55 cone into which the subject was instructed to thrust his nose. The outlet tubes from all the various points of the apparatus were taken independently to a flue in which a fan maintained a draught to the outside air.

General constructional considerations

In all work of this kind great care is required to avoid accumulation of accidental contamination of any kind. This is most serious in those parts of the apparatus actually handled by the subjects, i.e., the smelling points, but it arises in time in all parts of the apparatus. It was, therefore, necessary to provide for removal of every part for cleaning, and ground glass joints were fitted which enabled every section of tubing to be removed and cleaned by chromic-sulphuric acid and then water, with subsequent baking in a stream of clean air. Dismantling was facilitated by erecting the diluting devices upon a framework of steel rods, from which all the components could be supported by clamps.

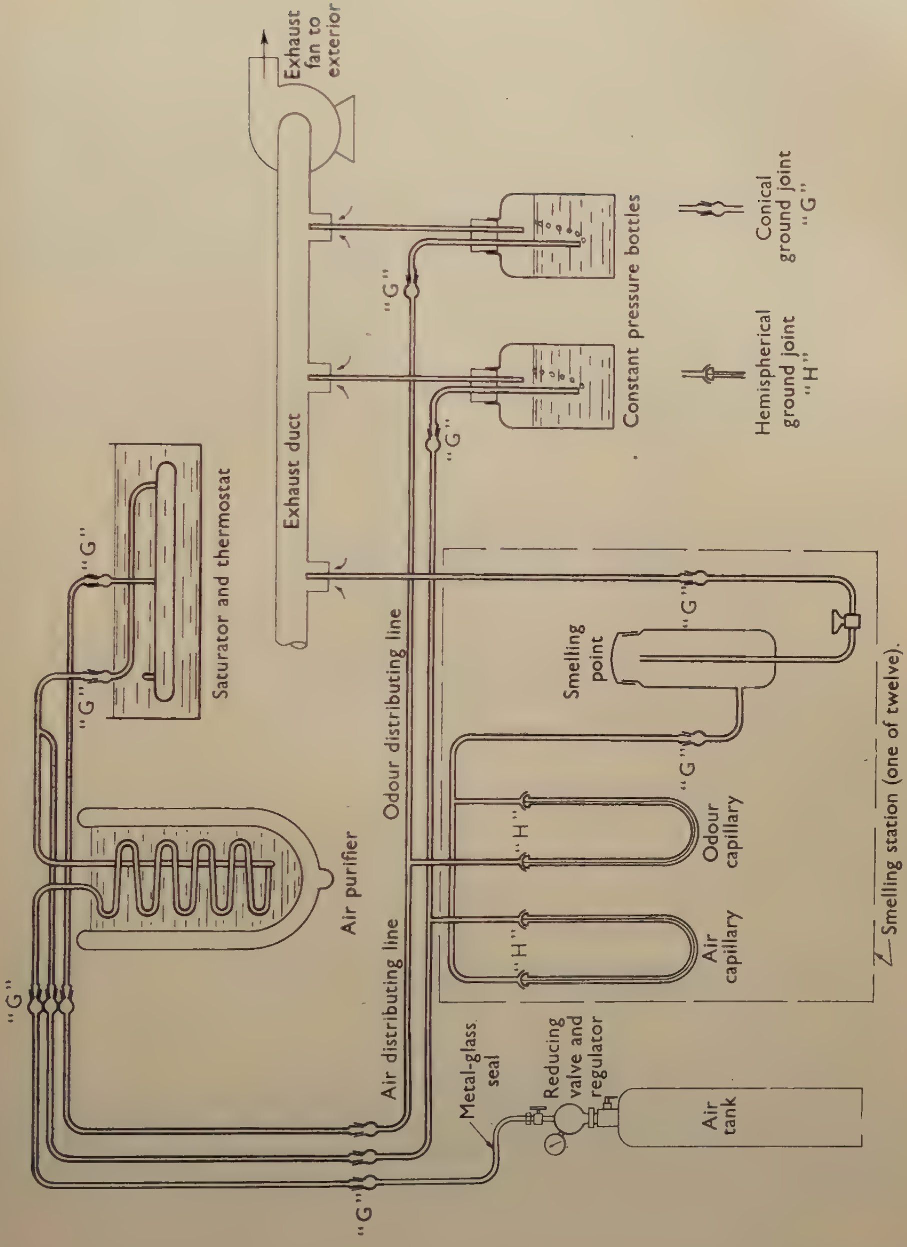
The arrangement of the apparatus can be seen from Figure 3. The steel frame supporting the dilution capillaries was erected along the centre of a long table, round the edge of which the twelve smelling points were disposed, so that the subjects could circulate from point to point round the table. It would undoubtedly be an advantage to separate the smelling points from the remainder of the apparatus by some form of opaque screen, but this was not feasible in the prototype.

Operation

Before each series of tests the entire glassware of the apparatus was dismantled and thoroughly cleaned with chromic-sulphuric acid mixture followed by copious washing with water. The parts were dried by heating in a current of air. The apparatus was assembled taking care not to handle any of the ground surfaces; a current of clean air was sent through every part of the whole apparatus, the saturator being temporarily replaced by a length of plain tubing. At this stage a blank experiment was made with a small group of subjects to establish that a satisfactory blank response level was attained. The saturator was filled and placed in circuit, and the thermostat set to the predetermined temperature. Then the air flow was started and adjusted, and the apparatus allowed to settle to a steady state before testing was commenced.

The group of subjects was stationed at the smelling points around the testing bench, and they were instructed to remove the cap and sniff each at his own point when a signal was given. After sniffing, each subject replaced the cap and moved forward to the next smelling point to await the next signal, which was given after a predetermined interval of time. In this way each subject tested all the 12 points as he circulated around the test bench and each subject tested all the concentrations of the series in the same sequence, though each subject entered the sequence at a different point. This procedure was exactly comparable with that previously used with the bottles containing aqueous solutions of the test substances. In subsequent tests it was quite easy if desired to alter the distribution of concentrations by re-arranging the capillary tubes.

No attempt was made to introduce a "masking odour" during these preliminary experiments—it was envisaged to build a similar but independent apparatus to provide masking stimuli at adjacent smelling points at a later stage. Apart from this, however, the choice of concentrations, the number of blank tests (zero odour concentration), the


Exhaust fan to exterior
Exhaust duct
Constant pressure bottles
"G"
"G"
Conical ground joint "G"
Hemispherical ground joint "H"
Saturator and thermostat
"G"
"G"
Odour distributing line
Smelling point
"G"
"G"
Air purifier
Odour capillary
"H"
Air capillary
"H"
Smelling station (one of twelve).
Air distributing line
"G"
"G"
Metal-glass seal
Reducing valve and regulator
Air tank

arrangement of report forms, and the instructions given to subjects, were arranged to follow as closely as possible to the pattern of the author's earlier experiments with the "sniff-bottle" technique (Cheesman & Mayne, 1953).

Performance

The behaviour of each portion of the apparatus was studied quantitatively during its development, and it is believed that the complete apparatus achieved the objective of providing a series of accurately controlled and steady concentrations of odorous vapour. Unfortunately, circumstances arose which necessitated dismantling it before it had been possible to make an extended series of replicated threshold measurements in order to assess directly the precision with which they could be measured. Apart from a number of smelling trials of a restricted nature (in which the efficiency of various air-cleaning arrangements were tested) five full olfactory tests were made, involving about fifty subjects. The results, set out in Table II, show a disappointing range of variation; there is insufficient basis for hazarding any opinion as to whether the variation is significant or not, or whether it can be correlated with any other variable. However, the working of the apparatus was sufficiently smooth to encourage confidence in the belief that it can prove a valuable instrument for future work, and the present account is published in order to place on record information which may be of use to other workers who might be contemplating the design of similar apparatus.

TABLE II

OLFACTORY THRESHOLDS OF *iso*-PROPANOL

Saturator at — 25° C. (v.p. = 1·0 mm. Hg. I.C.T.)

Test No.	*No. of subjects*	*Room Temp.*	*Threshold**	
			Scale steps (÷ 2) below saturation	*Partial pressure (mm. Hg.)*
1	20	16·0	3·8	0·072
2	10	18·3	6·2	0·013
3	10	14·9	4·9	0·034
4	10	11·0	3·5	0·088
5	10	15.0	4·5	0·044

* Threshold = concentration at which no. of positive responses = no. of negative responses.

Our warmest thanks are due to Professor R. C. Oldfield, at that time head of the Psychology Department, University of Reading, who provided a location for the apparatus and helped generously with every aspect of the work itself. The expenses of the research were met by grants from Unilever Ltd., the Medical Research Council and the Carton Association, to all of whom we record our thanks.

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BOOK REVIEWS

Perception and Communication. By D. E. Broadbent. London. Pergamon, 1958. Pp. v + 338. 55s.

The title of this book does some injustice to the importance of its contents. "Communication" hints at lengthy mathematical argument, yet the book is, in a very basic sense, non-quantitative. Nor is it limited to what is usually understood by "perception." Its early pages promise that it will do for hearing what was done for seeing by Vernon's *Further Studies in Visual Perception,* and granted the paucity of knowledge about auditory perception (as distinct from the psychophysics of the ear) this promise is fulfilled. But the clue to the book is given by the following statement. "Computing machines operate on a binary basis; they have long-term and short-term stores; servo systems seek goals by negative feed-back. But it is often urged that these analogies are of no worth unless there is evidence that organisms operate in the same way: and it is sometimes assumed as a further step that no such evidence exists. This book has been intended to show that nervous systems are networks of the type shown . . ., and of no other type. As has been admitted, the evidence for parts of the system is weaker than that for others: but a general layout of this type is entailed by the detailed evidence about behaviour which we have surveyed" (p. 304). In short, the book is concerned with fundamental questions about the organization of behaviour and approaches these questions by tough-minded scrutiny of detailed experimental evidence. In this respect, it compares with the manifestos of Hull, Skinner, Tolman and Hebb—and it does not suffer by comparison. No psychologist can adequately discuss the contemporary treatment of basic psychological issues without having read this book. Furthermore, as the quotation indicates, Broadbent is no dogmatist: he asserts his conclusions modestly; acknowledges alternative interprerations frankly, and sympathises with approaches other than his own. Although he criticises the theories of others, particularly S–R theory, his approach is wholesomely constructive and free from the sneering and special pleading which mars so much theoretical psychology.

Like the earlier manifestos mentioned, the book reflects a *Zeitgeist*; in this case, the impact of cybernetic principles on psychologists' ways of thinking about their subject matter. Also, it is based on a large accumulation of experimental evidence with the production of much of which the author has been intimately associated. Here, the evidence comes from work on conditioning, maze running, and rote learning; but in the main it comes from studies of multi-channel listening, vigilance and tracking tasks, the effects of noise, immediate memory, and the refractory period—that is, from those studies of skilled performance which have concerned the Cambridge Applied Psychology Research Unit. To all except a few psychologists, these latter studies are either wholly unfamiliar or known only by name as being of importance in the applied field of human engineering. Broadbent has done a service in providing the first easily accessible summary of these studies and it is safe to predict that his surveys will provide much material for the introductory textbooks of a few years hence: this being so, it is well that these surveys are both informed and lucid. (It would be admirable if Broadbent himself wrote another book for the more general reader covering the topics of vigilance, noise, multi-channel listening, and immediate memory.) It is, incidentally, pleasing to find a book of this calibre relying so heavily on British work. At a rough count, 40 per cent. of the more than three hundred references are to British papers. Noteworthy too is the fact that the book supplements and is supplemented by two other recent and outstanding books of Cambridge origin, namely, Bartlett's *Thinking* and Welford's *Ageing and Human Skill.*

Broadbent's purpose is not, however, the mere reporting of interesting and little known work; it is the critical assessment of this work in order to arrive at general principles of behaviour. This purpose does not become apparent until the book is well under way; and the reason for this is given in a cogent discussion of the limits of the hypothetico-deductive method (pp. 307–313). This revered method, it is argued, is bad strategy for scientific exploration and, at least in the early stages of research, the half-split method is more likely to pay dividends. Research should not start with specific, quantitative hypotheses, make deductions from these and then test the deductions against evidence. Rather, it should survey a population of empirical findings, establish generalizations about these, then consider broad qualitative classes of theory which might explain the generalizations, and eliminate those classes which do not accord with further empirical data. The reviewer is in substantial agreement with this argument. But the strategy of research and the expository structure of a book are different matters and the hypothetico-deductive sequence is certainly easier to follow in the latter case even if it does mislead some readers about how research is conducted. Broadbent, however, has chosen to write his book as he conducts his research. And it is this which gives the book its unusual and distinctive flavour, its main strength, and its main weakness. It starts with descriptions of empirical findings and ends with twelve principles and a "tentative information-flow diagram for the organism, as conceived at the present time," keeping a sharp eye all the time on the possible physiological processes underlying these principles. All this gives the stimulating feel of research in progress but makes it difficult for the reader who is unsophisticated in experimental psychology and, more important, makes for a lack of coherence among the different chapters for, while each chapter is excellent in itself, the whole does not impel the reader towards general theoretical conclusions in the way Broadbent obviously hoped it would.

The unusual plan of the book (coupled with its author's great competence and open-mindedness) also makes a reviewer's task difficult. Again and again, this reviewer was on the point of raising some query or objection only to find himself forestalled by the next paragraph. The limitations of the final conclusions are so candidly stated that any brief critical assessment becomes impossible. This does not mean that these conclusions are unworthy of serious consideration. Far from it. They draw attention to properties of the behaving organism which would seem, in most cases, to be of basic importance. Nor does it mean that the book lacks good things: it bristles with them and makes difficult the task of selecting among them (because of this, the book deserves a much fuller index than it has).

For the reviewer, the importance of the book lies less in the formally stated conclusions of the last chapter than in the fresh views constantly afforded of so many familiar (and unfamiliar) psychological phenomena and hypotheses. In this respect, it has "something for everyone." For the student of conditioning and learning, the inhibition theory of extinction is shown to be, at the least, unsubstantiated and a feasible alternative theory is suggested. Those interested in rote-learning will be stimulated by the excellent discussion of immediate retaining and short-term storage systems. For students of personality, the unexpected chapter on individual differences relates Eysenck's "introversion-extraversion" to the promising notion of long-sampling and short-sampling of information: and for the mental-tester, this same chapter throws out a potentially revolutionary hint by indicating that student success in the Cambridge Tripos may be predicted better from performance on the triple tester than on the AH4. The attention of students of perception and thinking is commanded by: the notion of "filtering"; the emphasis on the centrally coordinated, dynamic, and sequential relatedness of psychological activities; and the dramatic observation that intro- (or more properly retro-) spection is limited because it would otherwise represent the transmission of more information than did the original performance. For developmental and comparative psychologists, the chapter on "Verbal and Bodily Response" excites because of its comments on the nature of verbal behaviour and the new look it takes at stimulus-response relationships: the "three modes of response to a sequence of stimuli" shown in Figure 1 embrace a vast array of developmental phenomena, for example, they seem to spell out and partially account for the broad sequence shown by the development of young children's remembering performances. For the physiological psychologist there is an abundance of interest. And those of a philosophical persuasion will find much in the discussion of scientific method, a discussion which is all the more important since it comes from a productive experimentalist rather than a passive scholar of methodology.

It is at once a criticism and a compliment to wish that Broadbent had followed further the implications of these fresh viewpoints. For example, "attention" is considered in terms of a perceptual system which is viewed as a channel of limited capacity preceded by a filter. This formulation makes intelligible the phenomena of multi-channel listening and the effects of noise and also shows a distinct conceptual advantage over earlier formulations employing statements such as "a person can attend to only one thing at a time." This is no mean achievement. On the other hand, how would this formulation measure up to "attention" as manifested in situations more complex than those considered in the book? In the wider context, would "filtering" lose its usefulness or even become a hindrance? Broadbent does not tell us enough about the nature of his "filter" to enable us to say. Similarly, the concepts of "limited capacity channel" and the "short-term store" will certainly require elaboration before proving their theoretical mettle as will the difficult question of just how information is transferred from short-term into long-term storage. These key concepts are undeniably useful in the present context but they have within them the seeds of verbal magic. The book's characteristic of pursuing a topic only so far is illustrated again by the treatment of serial position effects. After an excellent discussion of order effects in certain immediate memory situations, the question of order effects in rote-learning is raised but is stopped short at an ingenious interpretation of reminiscence as due to "blocks" in a vigilance-type task. The reviewer would have liked to read Broadbent's fuller views on the factors responsible for order effects in rote-learning and, particularly, for the difference in order effects between immediate recall of sequentially unrelated items (e.g. nonsense syllables) and of items which are related in sequence (e.g. descriptive phrases in a prose passage). To mention this sense of incompleteness is perhaps unfair, for it arises not from a job badly done but from a job done so impressively well that the reviewer feels Broadbent could have filled these gaps had he put his mind to it—and that he probably would have done so had he cast his book into a more hypothetico-deductive form. Perhaps too, asking him to fill such gaps is asking him to supply more than present psychological knowledge will allow. Certainly it is asking that he should have written a book of another kind. So it is best to respond to the rich fare he offers not by asking for more but, rather, by gratitude for receiving so much that is original, illuminating, and indicative of further experimental work.

In summary, this is a first-rate book warmly to be recommended to all psychologists. Its conclusions are of great interest and will certainly lead to much fruitful discussion and experiment. Even more important are the lucid summaries of little known work and the novelty, ingenuity, and honesty of the viewpoints emerging from the text. They show that, whatever the future of communication theory, it will have contributed some rewarding additions to the conceptual equipment of even the most nonmathematical of psychologists.

I. M. L. HUNTER.

The Neurological Basis of Behaviour. Edited by G. E. W. Wolstenholme and Cecilia M. O'Connor. A Ciba Foundation Symposium. London. J. & A. Churchill Ltd., 1958. Pp. xii + 400. 52s. 6d.

The purpose of this symposium organized by the Ciba Foundation was to discuss the relationship between neurophysiology and behaviour. The majority of the participants were scientists working primarily with neurophysiological techniques, and consequently the proceedings have a distinct bias towards neurophysiology, and the behavioural aspects of the subject seem to have disappeared into the background. Modern neurophysiology is very much concerned with investigating the functions of sub-cortical and limbic lobe structures, and most of the papers contained in this symposium revolve around the investigation of these structures. Consequently there is an almost total neglect of the neocortex. It is also somewhat surprising that so few psychologists were invited to take part in a symposium which claimed to have as its aim the attempt to reach a closer understanding of behaviour. On reading the proceedings one is left with the impression that the physiologist may get into great difficulties when talking about behaviour. The presence of more psychologists might not only have helped to overcome some of these difficulties, but might even have been stimulating to the physiologists.

In the space available it is possible only to mention the few papers which have a direct relevence to psychology. Gastaut reviews some of the findings on the modifications of cerebral activity brought about by conditioning and the changes in conditioned reflex activity caused by cerebral ablation and stimulation. In an attempt to find the locus of conditioned "closure" he indicates the importance of the thalamic portion of the reticular formation of the brain. On reading his paper it seems that perhaps it is becoming possible to integrate data obtained from recording the electrical activity of the brain into the general body of psychological theory. This is further supported by Jasper's account of how he recorded the electrical activity of single nerve cells in the cerebral cortex of monkeys during the development of Pavlovian conditioned reflexes.

Dell links up neurophysiology with ethology in an attempt to demonstrate the physiological mechanisms that underlie "appetitive phases" of behaviour. He discusses evidence which suggests that an increase in the adrenaline content of the blood leads to increased activity of the brain stem reticular formation which in its turn might result in increased exploratory behaviour. Much of this work is speculative, but it does lead to certain predictions which could be tested using behavioural techniques.

Amongst the papers more directly related to psychology, Olds describes his experiments on "self-stimulation" in rats, which have led him to conclude that there are positive and negative motivation systems located within the brain and further that the cells of the positive system are differentiated into separate anatomical systems sensitive to different drives. Richter describes the physiological and behavioural symptoms of stress induced in rats by forced swimming in water for long periods of time. His findings lead him to suggest that severe stress results in lesions in the central nervous system. The forced swum rats have various abnormal cycles of activity and are similar in some ways to "hyperphagic rats." Richter suggests that the lesions may be in the hypothalamus. However, when this symposium was held no histological data were available.

Harris, Michael and Patricia Scott describe their beautiful studies of the effect of oestrogen on the sexual behaviour of the female cat. They show that implantation of stilboestrol dibutyrate into the posterior hypothalamus of spayed cats leads in the majority of cases to the full development of sexual behaviour, although the genital tract may remain anoestrous. Control implants into other parts of the central nervous system and subcutaneously did not result in mating.

Amongst the other papers Rosenzweig, Krech and Bennett discuss the correlation between the brain cholinesterase content of different strains of rats and the "adaptive" behaviour of these rats in the Krech Hypothesis Apparatus. Kluver reviews some of the recent work on the temporal lobe, and Green reviews work on the rhinencephalon. Penfield describes the "psychic" effects which he obtains when electrically stimulating the human temporal lobes. Brierley and Elisabeth Beck discuss the effects of destroying the dorsomedial and anterior thalamic nuclei in cats and monkeys. They consider that the syndrome resulting from complete destruction of the dorsomedial nucleus in the monkey is similar to the frontal lobe syndrome. However since there was no detailed psychological testing in their study this view can hardly be accepted at the moment.

There seems to be no unified purpose in this symposium and, apart from a few notable exceptions, the majority of the authors have been content with stating their experimental findings or reviewing previous work. Only occasionally does anybody go beyond this towards expressing new ideas, or indicating what lines of research might be pursued most profitably. If I may be allowed to coin a phrase I would suggest that this is because at the present time there is no adequate "neuro-behavioural" theory to provide lines along which integrated research can take place. Perhaps a development along the lines which Gastaut has suggested might fulfil this need.

Nearly all the work published in this symposium has been published elsewhere, the only difference being that each paper here is followed by a discussion. In most cases this discussion is inadequate, perhaps because there was not sufficient time provided, perhaps because the range of topics was too big. Having gathered together a number of leading workers in the fields of neurophysiology and behaviour, it seems a pity that the opportunity to discuss the most basic problems was not taken.

J. M. Oxbury.

Attitudes et Mouvements. By F. J. J. Buytendijk. Étude fonctionnelle du mouvement humain. Préface de E. Minkowski. Bruges. Desclée de Brouwer. 1957. Pp. 494. 35s.

Professor Butendijk's latest work is a kind of natural history of movements. In the opening section, he deals with the definition and classification of movements, with the intention of providing a "functional theory" of their nature and organization. (What however this theory is remains somewhat obscure.) Bodily attitudes and locomotion are next considered, with special reference to "preferred" attitudes and movement patterns. The author then provides a detailed analysis of some typical simple human reactions, including blinking, hand-withdrawal, scratching, grasping and the maintenance of equilibrium. This is perhaps the best documented and most valuable section of the book. It is followed by further sections dealing with expressive movements, the ontogenesis of movement (pre- and post-natal), and some characteristics of movement in childhood, maturity and old age. (Curiously, hand preference and dexterity are scarcely mentioned.) Typological implications are considered in the final section.

In many ways this is a valuable and timely survey. Progress in psychological work has long been impeded by lack of proper understanding of movement, its development, organization and modification. Indeed our ignorance is only too commonly cloaked by the bland use of terms such as "reaction" or "performance," which enable us to handle the outcome of movement in an experimental setting without troubling about its nature. Neurologists, too, have been hampered in their analysis of defects of movement, such as the apraxias, by lack of agreement as to what really constitutes a movement and how movements should be classified. (Indeed a leading neurophysiologist has recently remarked that the longer he explores the effects of cortical stimulation, the more uncertain he becomes as to what a movement really is.) Although Professor Buytendijk's work does not clear up very much, it at least serves to recall our attention to these basic issues.

It is a pity that Professor Buytendijk makes so little reference to experimental studies of skilled movement or to the kinds of problem which might be resolved by appropriate experiment. Further, his broad and discursive style, together with an irritating penchant for *Naturphilosophie*, may well discourage the experimental psychologist from reading his book. None the less, descriptive psychology of this kind still has its value and may well suggest new fields for detailed experimental analysis. It is therefore to be hoped that this scholarly study will not go unremarked.

O. L. ZANGWILL.

Diagnostic Performance Tests. A Manual for Use with Adults. By Boris Semeonoff and Eric Trist. London. Tavistock Publications Limited. 1958. Pp. xv + 176. 32s. net.

A high proportion of the data and most of the thinking in this book derive from work done during the 1939–45 war, and it represents, historically speaking, one of the first attempts to use cognitive tests in a richer and more meaningful way, as an important method in the study of personality.

For the authors, the testing situation is a social one where the psychological resources of the tester are importantly involved. It is a two-way situation and not just a psychometric assessment. This does not imply that the authors relinquish psychometric checks, but that they offer what is basically psychological method, within a background of scientific method and interpretation.

The book is a manual for the diagnostic use of performance tests with adults. A generalized rationale of the testing method is given briefly, and the development of the various techniques in their operational setting is described throughout several chapters. Further chapters contain information on norms and other statistical data. Trist pioneered the work at Mill Hill Emergency Hospital. He then became chief psychologist at the War Office Selection Boards, and when the organisation of the latter extended into a Special Assessment Board of the Inter-Services Research Bureau, Semeonoff became the officer responsible for the development of the material and the technique, collected fresh data after the war at Edinburgh University, and produced the present text at the request of the Tavistock Institute of Human Relations.

The five performance tests, each occupying a separate chapter, include two tests of conceptual thinking (Semeonoff-Vigotsky, and Trist-Hargreaves), an adaptation of Kohs Block Designs for diagnostic use (Trist-Misselbrook-Kohs), and new treatments of Alexander's Passalong Test, and the Carl Hollow Square Test.

Following the main chapters of the book are Appendices which outline allied techniques, not in the category of performance tests. These were grounded in Service requirements, and are part of hitherto unpublished work from this source. They are included because of their appropriateness to civilian employment, just as the performance tests are similarly applicable. The Mitchell Vocabulary Test, for example, is especially interesting.

The performance tests are fully described, each according to its appropriate usage. The opportunities given for observation and interpretation of individual behaviour, which are not possible in group testing, are discussed fully and are indeed a main purpose of the book.

Because of the fullness of the material, one very valuable aspect of the book is that the reader can see what amounts to research as it is taking place. The suggested interpretations are not dogmatic, and at several points opportunities arise for the reader to develop his own ideas. One might call it a refreshingly open-ended book, having something of the quality associated with the testing standpoint of the authors.

It is not a book for beginners, but it could be a very important undergraduate and postgraduate text, and an absolute "must" for anybody contemplating research in this field.

FLORENCE E. MACNEILL.

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